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THE WEARING POWER OF STEEL RAILS IN RELATION TO THEIR CHEMICAL COMPOSITION AND PHYSICAL PROPERTIES.

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(Continued from page 196.)

Turning our attention now again to the tables before referred to, I think that, notwithstanding we do not find absolute uniformity, as has just been explained, yet no one can fail to observe, in general, a difference, both in physical properties and in chemical composition, between the slower-wearing and the more rapid-wearing rails. Thus, in regard to the physical properties, I think it is entirely evident to inspection, that, in the torsion tests, the slower-wearing rails in each group, except perhaps on the high sides of curves, are characterized in general by lower height and greater length of diagram. In the tensile tests, again, the slower-wearing rails are, in general, characterized by lower tensile strength and greater elongation than the more rapid-wearing ones. In the shearing tests, the same thing appears, viz., in general, lower shearing stress and greater detrusion. And in the bending tests we see the same result, perhaps more strongly than in

any of the other tests, viz., that the slower-wearing rails are, in general, those which have the lower bending stress and the greater amount of deflection before rupture. In density, as would be expected, the slower-wearing rails have the greater density. Again, in the chemical composition, the same thing is observable, viz., that the slower-wearing rails are characterized in general by the lower amounts of the substances determined, carbon, phosphorus, silicon, and manganese, while in phosphorus units, which, as you remember, I suggested as an attempt to measure, in a degree at least, the combined influence of these substances on the steel, the same thing may be seen, viz., in general, the slower-wearing rails are characterized by the lower numbers of phosphorus units.

Now, how shall we render evident to ourselves, in a more tangible manner, what seems, on inspection, to be the general teaching of the results we are studying. I know of no method except that of combining these results together and forming averages. This, I believe, is the universal method applicable to all cases where it is desired to draw conclusions from results embracing a number of samples. If, therefore, we make a series of averages of the results obtained, the individual peculiarities of the different rails will, to a certain extent, disappear, and the general conclusions become more evident.

Accompanying Plate 8 gives a number of such averages. The first line on this plate gives the averages obtained from combining together all the essential data of the eight slower-wearing rails of the first group. The second line gives the same data for the eight more rapid-wearing rails of the same group. The next ten lines give the corresponding averages obtained from the remaining five groups. Turning our attention now to these averages, I think the same general conclusions which we have already obtained become still more clearly evident, viz., that, perhaps, with the exception of the rails on the high sides of curves, the slower-wearing rails in each group are in general characterized by those peculiarities which we are accustomed to comprehend under the general term softer steel; that is, in general, diagrams of less height and greater length, lower tensile strength and greater elongation, lower shearing stress and greater detrusion, lower bending stress and greater deflection, and lower amounts of carbon, phosphorus, silicon, and manganese, and lower phosphorus units. The exceptions to the law, if we may so call it, will be readily noticed, and are, perhaps, inseparable from averages formed from so small a number

of samples as are some of these. The rails on the high sides of curves will be noticed further on.

With regard to the averages under consideration, it is perhaps interesting to notice that the slower-wearing rails are characterized throughout the whole series by lower height of diagram and lower elastic limit in the torsion tests, by lower elastic limit in the annealed tensile tests, by lower bending stress and greater deflection in the bending tests, by lower phosphorus and lower manganese, in the chemical analyses, by lower phosphorus units and by greater density. It should be remarked that the high average silicon of the slower-wearing rails on grade tangents is caused almost entirely by rail No. 881, a rail whose chemical composition and physical properties are certainly anomalous. This rail has 0·48 of silicon together with high carbon and manganese, and yet it gives with high tensile strength, as would be expected, a very good elongation. I may add that three determinations of the silicon in this rail were made, in order to be sure that there was no error in the work. The peculiarities of this rail cause the average of which it forms a part to differ from the other averages of the slower-wearing rails in several particulars. If this rail is thrown out, and an average made from seven rails instead of eight, all the data fall more nearly into line with the averages of the slower-wearing rails in the other groups. Thus, to give two or three examples, the average tensile strength of the unannealed tests becomes 75,143 lbs. instead of 79,625 lbs.; the elastic limit of same tests, 35,000 lbs. instead of 37,625 lbs.; the tensile strength of the annealed tests, 75,428 lbs. instead of 78,250 lbs.; the carbon, 0·301 instead of 0·324; the phosphorus, 0·082 instead of 0·076; the silicon, 0·048 instead of 0·102; the manganese, 0·530 instead of 0·563; and the phosphorus units, 31·3 instead of 34·8

With regard to the rails on the high sides of curves, it will be observed that they do not seem to be governed throughout by the same law as the remaining groups. Whatever may be the reason for this no one can fail to observe indications, which seem to point in the direction, that on the high sides of curves the harder rails give the better wear. This is especially noticeable in the group of rails on the high sides of level curves. It is, nevertheless, to be observed that in some of the data, especially height of diagram and elastic limit in the torsion tests, elastic limit in both the annealed and unannealed tensile tests, in the shearing tests, in the deflection of the bending tests, and

in the chemical composition the groups of rails on the high sides of curves do not differ from the remaining groups. It would not be surprising if more extended data should show that a different quality of steel would give better wear on the high sides of curves than was found best in the other conditions of service. For on tangents, and on the low sides of curves, the kind of wear to which the rails are subjected seems to be different in its nature from the wear on the high sides of curves. On tangents, and on the low sides of curves, the loss of metal is apparently due to two causes: 1st and principally, to rolling friction; and 2d, to the sliding of the wheels. Now on the high sides of curves, in addition to the loss of metal due to these causes, there is also that due to the flanges of the wheels, or flange wear. Now flange wear may not only be different in its nature, but may also require different qualities in steel to most successfully resist it from what are found best where the rails are subjected principally to rolling friction. This point will be referred to again when we come to speak of the nature of wear.

Thus far we have confined our study to the consideration of averages obtained from the rails within each group. But, if I am right, the value of averages increases with the number of samples from which they are derived. It will be instructive, therefore, to make averages of the slower and faster-wearing rails, embracing as large a number of samples as possible. But, as has just been noticed, the kind of wear to which rails have been subjected, whether rolling and sliding friction or flange wear, may have an influence. It will, therefore, perhaps be most instructive to make averages of all the rails which have been subjected to the same kind of wear, and see what their testimony is as to the physical properties and chemical composition of the slower-wearing rails. Now, forty-eight rails out of the sixty-four we are studying did their service under similar circumstances as to kind of wear; that is, forty-eight of the rails were on tangents and on the low sides of curves, and consequently had little or no flange wear, while sixteen of the rails were on the high sides of curves, and were consequently subjected to both kinds of wear.

In accompanying Plate 8, line No. 13 gives the averages of the twenty-four slower-wearing rails which did their service on tangents and on the low sides of curves, and were consequently subjected to the same kind of wear; while line No. 14 gives the averages of the twenty-four faster-wearing rails that did their service under the same con-

ditions. Line No. 15 gives the averages of the eight slower-wearing rails that were on the high sides of curves, and line No. 16 the averages of the eight faster-wearing rails under the same conditions. What, now, do these four lines of results teach?

With regard to the first two of these four lines, inspection seems to me to show very clearly that the slower-wearing rails are characterized by those physical properties and that chemical composition which we are accustomed to comprehend under the term softer steel. In other words, we find that the slower-wearing rails are characterized by lower height and greater length of diagram and lower elastic limit in the torsion tests; by lower tensile strength, lower elastic limit and greater elongation in the tensile tests; by lower shearing stress and greater detrusion in the shearing tests; by lower bending stress and greater deflection in the bending tests; by lower carbon, phosphorus and manganese, with a trifle higher silicon (caused by rail No. 881, as has been before explained) in the chemical composition; by lower phosphorus units, and by greater density.

With regard to the next two lines, giving the averages of the rails on the high sides of curves, the testimony of the data is not uniform. The length and area of diagram in the torsion tests, and the elongation of the tensile tests, and also in a slight degree the carbon, indicate that the slower-wearing rails are the harder steel. In every other item of the data, however, the slower-wearing rails are characterized by the same peculiarities that we have noticed in regard to the other rails, viz., lower height of diagram and lower elastic limit in the torsion tests; lower tensile strength and lower elastic limit in the tensile tests; lower shearing stress and greater detrusion in the shearing tests; lower bending stress and greater deflection in the bending tests; lower phosphorus, silicon and manganese, with lower phosphorus units in the chemical composition, and greater density. It may be that this want of uniformity in the data from the rails on the high sides of curves is due to the fact that our averages only embrace sixteen samples, and it may be, on the other hand, that an indefinite number of samples would give the same results. Or, again, it may be that this want of uniformity is due to a fact that has already been stated, viz., that on the high sides of curves the loss of metal is due to a mixed wear, partly from rolling and sliding friction and partly from flange wear. If a certain quality of steel most successfully resists loss of metal due to rolling and sliding friction, and a certain other quality of steel best

resists flange wear, then certainly it is not strange that in results obtained from a series of rails, which have been subjected to both these kinds of wear at once, there should not be absolute uniformity. If in the case of every rail the loss of metal was due almost entirely to rolling and sliding friction, or, on the other hand, if the loss of metal was due almost entirely to flange wear, there is very little doubt but that the law would appear. But it is not difficult to conceive that there may be in any series of rails such a relation between the loss of metal due to both of these causes as to make it quite difficult to discover what kind of steel is best adapted to the high sides of curves.

Nevertheless, in an investigation of the extent of the one under consideration, perhaps the best that we can expect to do is to get indications, and as far as may be definite conclusions, as to the kind of rail best adapted to resist wear on the road taken as a whole. It will, perhaps, therefore, not be wise to discuss further the peculiarities of steel rails best adapted to any one kind of service, but to ask: What is the teaching of all the results we have obtained? This teaching will of course become evident if we make two averages, the one of the slower-wearing and the other of the faster-wearing rails, the two embracing the whole sixty-four rails.

In accompanying Plate 8, lines Nos. 17 and 18 give such averages. An inspection now of these two lines seems to me to present in a very striking light the differences in the results of the physical tests and in the chemical composition, which are characteristic of the slower-wearing and the more rapid-wearing rails. In every particular of physical test and of chemical composition, except a trifle higher silicon, which, as has been before explained, is caused by rail No. 881, the averages of the 32 slower-wearing rails are such as belong to what we are accustomed to call softer steel. In other words, the slower-wearing rails have on the average less height and greater length of diagram, with lower elastic limit in the torsion tests; lower tensile strength, lower elastic limit and greater elongation in the tensile tests; lower shearing stress and greater detrusion in the shearing tests; lower bending stress and greater deflection in the bending tests; lower carbon, phosphorus, manganese and phosphorus units in the chemical composition, and greater density. It seems to me, therefore, that so far as we may trust the teachings of 64 rails, all of which have actually done service, we can hardly escape the conclusion that the softer rails give the better wear.

In view of this conclusion, two questions very naturally arise:

1st. If the softer rails give the better wear why not make rails even softer than those in the series we are now considering; or, in other words, what is the limit of softness to which it is possible to go?

And, 2d. With our present knowledge of steel, is there any conception which will at all assist us in understanding why it is that the softer rails give the better wear?

As to the first question, I think almost every practical man will say at once: If your conclusions are correct that the softer rails give the better wear, why not make rails as soft as possible? indeed, why not use iron rails instead of steel? In answer to this latter question I think it may be fairly said, that such metal as is used for rails which we are accustomed to call iron is quite a different metal from that commonly called steel which is used for rails. From the processes used in their manufacture, iron rails are never free from slag, and lack homogeneity of structure, while steel rails, from their method of manufacture, are almost entirely free from slag, and are, or should be, practically homogeneous throughout. In the puddling process by which the iron for iron rails is made, the metal when taken from the puddling furnace is a dripping mass of iron and slag, and the amount of slag that is removed is dependent, in great part at least, on the subsequent working. Furthermore, in the subsequent operations before the rail is finished there are plenty of chances for non-welding, so that an iron rail might almost be cited as an example of non-homogeneity of structure. In the processes by which steel rails are made, on the other hand, the metal previous to the formation of the ingot is in a state of fusion, thus allowing the almost complete separation of the slag, and the formation of ingots, from which rails are to be rolled, that are, or should be, practically homogeneous in structure. Now that rails made from ingot iron, if such a metal is possible, that is iron which before it became an ingot was in a state of fusion, would not, in actual service, wear slower and lose less metal per million tons' burden that passed over them than even the best rails in the series we are studying, is an assertion which I think few men would be willing to make, and fewer still would be able to maintain.

But, it seems to me, that there is a natural limit to the softness of steel for use in rails, which limit is determined by the service to which the rails are to be subjected. It is evident, I am sure, that besides their ability to successfully resist wear, and to withstand the shocks

and blows to which they may be subjected, rails have also another very important duty, viz., they must be able to hold up the load which is to pass over them without being deformed or squeezed out of shape by this load. They must also have sufficient stiffness so as not to be bent down between the ties by the load, and thus give rise to a series of elevations and depressions in the track. To take an extreme case for an example, if we had a rail made of lead no one doubts but that it would entirely fail to hold up the load between the ties, and would on the ties be deformed and squeezed out of shape, probably, by the first train that passed over it. Now those qualities of metal which enable it to hold up a load without bending, and to successfully resist deformation from compression, are generally comprehended with others under the term hardness. In other words, to apply this to the case we have in point, the harder the steel in the rails the less liable will they be to bend between the ties and to suffer deformation, or to squeeze out from the loads which are above them. Here, then, is the limit of softness. The steel in the rails must not be so soft that they will bend between the ties, or squeeze out of shape and become deformed from the compression due to the superincumbent load.

Just exactly where this limit shall be placed is of course a question to be decided by the weight of the locomotives and cars which are intended to run over the rail. On roads where the locomotives and cars are comparatively light the rails could be made softer, and consequently, if our conclusions are correct, the loss of metal per million tons' burden would be less than on roads using heavier locomotives and freighting their cars with heavier loads. Just where this limit of softness is in the case of the Pennsylvania railroad, it is of course impossible to say, but from an examination of all the rails which have been tested at Altoona, amounting to nearly a hundred samples, I think that steel which gives a tensile strength of 65,000 lbs. per square inch or over will give very little or no trouble from bending or deformation in the track. It may be that this limit might be still lower, but with our present knowledge of the subject, and without further experimentation, I should hardly like to recommend it. Another consideration comes in here, and that is, it is a little more difficult to successfully make soft steel than to make hard steel, and it may be, therefore, that the difficulties of manufacture will help to establish the limit of softness.

As to the second question: Is there any conception which helps us

to understand why it is that the softer rails give the better wear? I will say that in view of our absence of knowledge as to exactly what quality or combination of qualities of steel wear is a function of, the answer to this question becomes difficult. Nevertheless, we may possibly get a little help upon this point by devoting a few moments to the consideration of what takes place when steel suffers loss of metal by wear. What then is the condition of the surfaces involved in wear? What are the forces which act, and what are the strains produced?

If I understand the matter correctly, no two surfaces ever have been made, or can be made, that are absolutely smooth. At the very best, the smoothest surfaces are made up of elevations and depressions, very minute, it is true—perhaps almost infinitesimal and entirely incapable of measurement—but, nevertheless, real elevations and depressions. When these elevations and depressions are tangible we call the surface rough; when they are infinitesimal we call it smooth. If, now, this reasoning is correct, the surface of the head of a rail, as well as that of the circumferences of the wheels above it, are made up of elevations and depressions, which, when the two surfaces are in contact, must more or less fit into one another. And it is this fitting in of the minute elevations and depressions of the two surfaces that gives rise to friction. If the two surfaces were absolutely smooth, there would be no friction, and, consequently, no tractive power in the locomotives, nor would the wheels under the cars turn around. Friction of this kind we are accustomed to call rolling friction. In reality, then, a rail and the wheel which rolls above it may be regarded as a rack and pinion with infinitesimal teeth, lacking, of course, the element of regularity as to the position of the teeth which characterizes a rack and pinion. So much for the surfaces involved in wear.

Now, what are the forces involved in wear, and what strains do they give rise to? If I understand the matter rightly, there are two kinds of friction which may occasion loss of metal by wear in the case of rails: 1st. Rolling friction, which occurs when the wheels turn around and the trains move forward; and, 2d. Sliding friction, which occurs when the wheels turn around in the same place, as in the case of slipping drivers, or when the wheels do not turn around and yet move along the track, as in the case of sliding wheels. It is probable, however, that by far the largest portion of the loss of metal which

rails suffer is due to rolling friction; it will therefore, perhaps, be sufficient for our purpose to consider this case only.

The forces which act between the top of the head of the rail and the wheels in rolling friction may, it seems to me, be regarded as two in number. There is, first, a force acting directly downward, due to the weight of the locomotives and cars. This force may be regarded as a vertical force acting perpendicularly to head of the rail, and is in action both when the train is standing still and when it is in motion. Secondly, there is a force acting parallel to the head of the rail, due to the traction or impelling power of the locomotives. In the case of the driving-wheels, this force may be supposed to act in the direction opposite to that of the motion of the trains. In the case of all the other wheels, this force may be supposed to act in the direction of the movement of the trains, and it is antagonism to this force by the rail that causes all these wheels to turn around. In the case of the drivers, the amount of this force acting parallel to the head of the rail is sufficient to overcome the total train resistance; in other words, to cause the train to move. In the case of the other wheels of the train, acting individually, this force parallel to the head of the rail is small, being only that necessary to cause the wheels to turn around; in other words, to overcome journal friction. The force parallel to the head of the rail acts only when the train is in motion.

In the case of rolling friction, there is, of course, no wear except when both these forces are acting at once. But when two forces are acting at the same time their line of action must be regarded as in the direction of the resultant of the two forces. In the case we are considering we have a vertical force and a horizontal force acting at the same time, and the resultant line of action must, of course, be a diagonal. In other words, in rolling friction, if the conceptions given above are correct, the line of action of the force which produces wear must be regarded as a diagonal to the line formed by the head of the rail. In the case of the driving-wheels, the diagonal is inclined toward the front end of the train, and in the case of the other wheels it is inclined toward the rear of the train.

Returning for a moment to the conception previously mentioned, that the top of the head of the rail and the surface of the circumference of the wheel are a rack and pinion, with infinitesimal teeth, but without regularity in the teeth, let us see what kind of strain would be produced in these minute teeth by a force acting diagonally to the line

of the head of the rail. I hardly see how we can avoid the conclusion that this strain would be a bending strain. In the case when the infinitesimal teeth of the wheel and rail did not fit into interstices, but struck against each other, in which case the ends of the teeth only would be involved in the strain, then surely the strain would be a bending one; and again, in the case in which the teeth mutually engaged each other as a rack and pinion, so long as there was motion of the train, which is a necessary condition of rolling friction, it seems to me that the strain would still be that of bending.

If we are right in regard to the nature of the surfaces involved in wear and the strains produced, wear is simply the breaking or pulling off of the infinitesimal teeth by the strains to which they are subjected. And here we see why it is that the softer rails give the better wear, for the harder the steel the more brittle it is; and the more brittle the steel the more readily will these infinitesimal teeth be broken off by the strains applied. On the other hand, the softer the steel the more readily will these infinitesimal teeth bend and flatten down under the strain without breaking off. Or, to make the statement general, if the nature of the steel is such that under the strains applied, whatever they may be, the teeth readily break off, which is characteristic of hard steel, the more rapid will be the wear; if, on the other hand, the nature of the steel is such that the teeth readily suffer distortion without rupture, bend and flatten down without breaking off, which is characteristic of soft steel, the slower will be the wear. The above is not offered as a complete solution of the problem of wear, but as possibly helping us to understand why it is that the softer rails give the better wear as we actually find they do.

The data which have been obtained in the work done on this series of rails make it possible to get at some very interesting results as to the rate of wear of rails under different conditions of service. Thus, for example, what is the comparative rate of wear of rails on grade tangents and on level tangents, or on grade curves and on level curves, or on curves as compared with tangents, etc.? These comparisons may be made by taking the average loss of metal per million tons of all the rails which did their service under any one set of conditions, and comparing it with the average loss of metal of all the rails which did their service under any other set of conditions. This of course involves the supposition that the sixteen rails, for example, which did their service on grade tangents were equally as good, taken as a whole,

as the sixteen which did their service on level tangents. Whether this supposition is absolutely true or not, I think the differences between groups of rails, that is the differences between the groups that did their service under different conditions, can hardly be great enough to lead us into serious error. The results of these comparisons is given in the following three tables. In these tables the first column denotes the kind of service to which the rails have been subjected, the second column denotes the number of rails from which the average loss of metal per million tons in column third is derived, and the fourth column denotes the ratio of the loss of metal for the kind of service between which the comparison is made.

TABLE NO. I.

EFFECT OF GRADE ON WEAR.

Kind of service.	No. of rails.	Average loss of metal.	Ratio.
Level tangent,	16	·0384	1 : 1·82
Grade tangent,	16	·0701	
Level curve,	16	·0706	1 : 1·80
Grade curve,	16	·1277	
Levels,	32	·0545	1 : 1·81
Grades,	32	·0989	

The above table shows the effect of grade on the wear of rails. The average grade for the rails on tangents was 64·75 feet to the mile, and, as will be observed, the rails on the grades lost 1·82 times as much metal as the rails on levels. The average grade for the rails on curves was 53·13 feet to the mile, and, as will be observed, the rails on the grades lost 1·80 times as much metal as the rails on levels. The average grade for the whole 32 rails which were on grades was 58·94 feet to the mile, and, as will be observed, the ratio of the loss of metal on levels and grades is as 1 : 1·81. This table may be roughly summed up by saying that within the limits of our experiments, and with a grade of not quite 60 feet to the mile, the rails on the grade lose about 80 per cent. more metal per million tons than the rails on levels.

TABLE NO. II.

EFFECT OF CURVES ON WEAR.

Kind of service.	No. of rails.	Average loss of metal.	Ratio.
Grade tangents,	16	·0701	1 : 1·82
Grade curves,	16	·1277	
Level tangents,	16	·0384	1 : 1·84
Level curves,	16	·0706	
Tangents,	32	·0542	1 : 1·83
Curves,	32	·0992	

The above table shows the effect of curves on the wear of rails. The average degree of curvature of the rails on grades was $4\frac{7}{8}°$, and, as will be observed, the rails on the curves lost 82 per cent. more metal per million tons than the rails on tangents. The average degree of curvatures on levels was $2\frac{23}{32}°$, and, as will be observed, the rails on curves lost 84 per cent. more metal than the rails on tangents. The average degree of curvature for the whole 32 rails on curves was $3\frac{51}{64}°$, and, as will be observed, the rails on curves lost 83 per cent. more metal than the rails on tangents. This table may be roughly summed up by saying that under the conditions of our experiments, and on about 4° curves, the rails on the curves lose about 83 per cent. more metal per million tons than the rails on tangents.

TABLE NO. III.

WEAR ON HIGH AND LOW SIDES OF CURVES.

Kind of service	No of rails	Average loss of metal.	Ratio.
Grade curve, low side,	8	·0801	1 : 2·19
Grade curve, high side,	8	·1754	
Level curve, low side,	8	·0500	1 : 1·82
Level curve, high side,	8	·0911	
Low sides,	16	·0650	1 : 2·05
High sides,	16	·1332	

The above table shows the relative rates of wear on the high and low sides of curves. The average degree of curvature of the rails on the grades was $4\frac{7}{8}°$, and the average elevation of the rails on the high sides of the curves was $4\frac{7}{8}$ inches, and, as will be observed, the rails on the high sides of the curves lost 2·19 times as much metal per million tons as the rails on the low sides of the same curves. Again, the average degree of curvature of the rails on the levels was $2\frac{23}{32}°$, and the average elevation of the outer rail was $2\frac{23}{32}$ inches, and, as will be observed, the rails on the high sides of the curves lost 82 per cent. more metal than the rails on the low sides of the same curves. Finally, the average curvature of all the rails on curves was $3\frac{51}{64}°$, and the average elevation of the outer rail was $3\frac{51}{64}$ inches, and, as will be observed, the rails on the high sides of the curves lost 2·05 times as much metal as the rails on the low sides of the same curves. This table may be roughly summed up by saying, that under the conditions of our experiments, with an average curvature of about 4°, and an average elevation of the outer rail of about 4 inches, the rails on the high sides of the curves wear a little over twice as fast as the rails on the low sides of the same curves.

The data which have been obtained in the work done in this series of rails furnish likewise the means of getting at some interesting facts as to the life of rails. It is of course a difficult thing to say just exactly when a rail is so worn that it can fairly be called worn out. And yet we can perhaps make a few assumptions which, although not absolutely definite, may aid in throwing some light on this interesting question. If we take any standard 67 lb. rail, and suppose that the head is worn off on tangents or on the low sides of curves until it is from $\frac{1}{4}$ to $\frac{3}{8}$ of an inch lower than it was when rolled, the amount of metal worn off will vary from about 6 lbs. to 10 lbs. per yard. On the other hand, if the rail has done service on the high sides of curves, the amount of metal worn off when the side of the head is worn up to the fish-plate will not be far from 7 lbs. per yard. But as I understand it, the present practice on the road is, when a rail has become worn on the high side of a curve up to the fish-plate, to remove it to the other side of the curve, and get two or three years more wear out of it in that position. For the very general conclusions that we are after, therefore, it will perhaps be sufficiently accurate to say, that a rail may fairly be said to be worn out when it has lost 8 lbs. of metal per yard. This assumption of course does not pretend to actually represent the loss of metal by wear which may or ought to be obtained from every rail before it is worn out. In the case of the old standard section, it is probable that rails may fairly be said to be worn out when they have lost in the neighborhood of 8 lbs. per yard. In the case of the new standard section (section of 1875) a loss of from 10 to 14 lbs. per yard would leave the rail in as good condition as a loss of 8 lbs. per yard from the old standard section. In the following data, however, a rail is assumed to be worn out when it has lost 8 lbs. per yard, and from the figures given it will be easy to obtain the life of rails for any other assumed loss of metal. It will, of course, be borne in mind that the figures which follow are strictly applicable only to rails of the same quality and doing service under the same conditions as those from which these figures are derived. The conclusions must therefore be regarded as entirely general.

The following table shows what, with such average quality of steel as this report deals with, may be expected from rails in the various conditions of service. The first column gives the number of the line in the table, the second column the kind of service, the third column the number of rails from which the average loss of metal in column

four is obtained, the fifth column the number of million tons required in order that the rail may lose 8 lbs. of metal per yard, the sixth column the life, or number of years the rail would last in the north track, and the seventh column the life, or the number of years in south track. These last two columns are computed on the supposition that the yearly tonnage on the north track is 5,000,000 tons and on the south track 8,000,000 tons. On the north track, during the 9½ years previous to July, 1879, the average annual tonnage, including of course the locomotives and the weight of the cars as well as their loads, has been 4,904,000 tons, while on the south track, during the 6 years previous to July, 1879, the average annual tonnage has been 7,985,000 tons.

TABLE IV.

THE LIFE OF RAILS UNDER DIFFERENT CONDITIONS OF SERVICE.

No. of line.	Kind of service.	No. of rails.	Average loss of metal.	No. of million tons to lose 8 lbs.	Life in years, North track.	Life in years, South track.
1	Level tangents, . .	16	·0384	208·4	41·7	26·1
2	Grade tangents, . .	16	·0701	114·1	22·8	14·2
3	Level curves, . . .	16	·0706	113·3	22·6	14·1
4	Grade curves, . .	16	·1277	62·7	12·5	7·8
5	Low side level curves,	8	·0500	160·0	32·0	20·0
6	High side level curves,	8	·0911	87·8	17·5	11·0
7	Low side grade curves,	8	·0801	98·9	19·8	12·3
8	High side grade curves,	8	·1754	45·7	9·1	5·7
9	Tangents, ...	32	·0542	147·6	29 5	18·5
10	Curves, ...	32	·0992	80·6	16·1	10·1
11	Levels, ...	32	·0545	146·8	29·4	18·3
12	Grades, ...	32	·0989	80·9	16·2	10·2
13	Low side curves, . .	16	·0650	123·1	24·6	15·4
14	High side curves, .	16	·1332	60·1	12·0	7·5
15	All conditions, . .	64	·0767	104·3	20·8	13·0
16	32 slower-wearing, .	32	·0506	158·1	31·6	19·8
17	32 faster-wearing, . .	32	·1028	77·8	15.6	9·7

In the above table the first eight lines show what may be expected from rails of the quality we are considering in all conditions of service, on the supposition that a rail is worn out when it has lost eight pounds of metal per yard, and that it carries five million tons per year in the north track and eight million tons per year in the south track. It is, of course, very simple to obtain from the figures given the life of rails under any other supposition. Thus, if the tonnage doubles, the life of the rails would of course be one-half as great

as that given above. On the other hand, if a rail is not regarded as worn out until it has lost 12 pounds of metal per yard, the life of the rail, the tonnage being the same, will be one-half greater than the figures given above. The remaining lines of this table, except the last two, show the life of rails under different combinations of the various conditions of service. They are given for the sake of information, and perhaps do not need any special comment or explanation. The first eight lines, of course, give the data that are most valuable. With regard to these data it will be noticed that on the high sides of grade curves, the average degree of curvature being $4\frac{7}{8}°$ and the average grade 64·75 feet to the mile, a rail of such average quality as those we are studying, and on the suppositions above given as to loss of metal and tonnage, may be expected to last a little less than six years in south track and a little over nine years in north track. On the low sides of the same curves the life is a little more than twice as long. This table may, perhaps, be best summed up by saying that rails have their shortest life on the high sides of grade curves, next on the high sides of level curves, next on the low sides of grade curves, next on grade tangents, next on low sides of level curves, and their longest life, as would be expected, on level tangents.

It is an interesting question whether it will be possible to get the high number of years' service given under some of the conditions above in actual practice. At first sight this seems improbable. But so far as loss of metal by wear is concerned, I see no escape from the conclusions given above, the conditions remaining the same. Other causes may, of course, take a rail out of service. And undoubtedly these other causes, such as crushing, breaking, distortion of head, and especially uneven wear, will all combine to make the average life considerably less than the above figures show. Nevertheless, the life of rails due to wear alone being known, it will, of course, be the aim to diminish the removal of rails from the track due to other causes as much as possible, and thus approximate closely to the best results.

One word more in regard to the life of rails. The last two lines in the above table give the same items of information, computed in the same way, as are contained in the remainder of the table. But in the first of these two lines the data are such as would be obtained if all the rails in the series had been as good as the average of the thirty-two slower-wearing or better rails, while the second of these two lines shows what would result if all the rails had been the same as the average

of the thirty-two faster-wearing. As will be observed, if all the rails in the series had lost metal or worn as fast as the average of the thirty-two faster-wearing rails, the best that we could expect under the suppositions given, as to loss of metal and yearly tonnage, would be a tonnage of about seventy-eight millions and a life of a little less than ten years in south track. On the other hand, if all the rails had been as good as the average of the thirty-two slower-wearing or better rails, the tonnage and life would be just about double these figures. In other words, the slower-wearing rails are on the average about twice as good in quality as the faster-wearing, and would consequently give about twice as high a tonnage and twice as long a life.

These data may be put in another form, which perhaps will be the fairer way of looking at them. The average life of the sixty-four rails we are studying, on the supposition that they are worn out when they have lost eight pounds per yard, and that the yearly tonnage in south track is eight million tons, is, as is seen in third line from bottom of above table, thirteen years. If, now, we were able to obtain steel rails as good in average quality as the thirty-two slower-wearing rails in our series, this average life would be almost twenty years, or an increase in life of over 50 per cent.

The question now arises, How shall the results of this work be utilized? I think it is entirely evident that the direction in which effort must tend in order that these results may be utilized is toward securing softer steel. And just here, perhaps, is the proper place to notice what seems to me a very important point. As you know so well this is our second investigation upon steel rails. The first investigation, as has already been stated, dealt principally with the question as to the relation between the chemical and physical properties of steel rails and their power to resist crushing and fracture in track. The conclusion reached, as you remember, was that the softer rails are less liable to crush or break in service than the harder ones. Now in this second investigation we find that the softer rails give the better wear. In other words, the softer steel makes rails which are not only less likely to crush or break in service, that is, rails that insure greater safety in the track, but also the softer steel makes rails that give better wear. And, unless the conclusions upon these two points can be overthrown by an equal amount of work, as carefully and conscientiously done, I do not see how the Pennsylvania railroad can, in the future, do otherwise than use every effort to secure softer steel in its rails.

And for this purpose I can see nothing better than that for the future the rails shall be bought on specifications, and subject to inspection and test before they are accepted. What specifications, therefore, does it seem wise to propose?

1st. As to chemical composition. It seems to me that it would be entirely philosophic to take as the specifications for the chemical composition the average analysis of the 32 slower-wearing rails in the series we are studying; especially as this analysis confines the work done on the 25 samples of steel rails in the first report to you on this subject. This average analysis is as follows:

Carbon,	0·334 per cent.
Phosphorus,	0·077 "
Silicon,	0·060 "
Manganese,	0·491 "

But, as was stated in that report, the amount of phosphorus above given is rather lower than the manufacturers of steel rails in this country can comfortably work. It would perhaps, therefore, be wise to increase the amount of phosphorus a little and diminish some of the other hardeners proportionally. In view of this reasoning, therefore, I see nothing better than to re-recommend the specifications as to chemical composition in our first report, as follows:

Phosphorus, not above,	0·10 per cent.
Silicon, not above,	0·04 "
Carbon, between 0·25 per cent. and 0·35 per cent., with an aim at,	0·30 "
Manganese, between 0·30 per cent. and 0·40 per cent., with an aim at,	0·35 "
Sulphur and copper,	No specifications.

2d. As to physical tests and inspection. What physical tests and inspection shall the Pennsylvania railroad prescribe for its rails? The inspection which is now practiced seems abundant to exclude rails whose defects are evident to the eye. As to the quality of the steel something further seems to be needed. Now, a physical test to determine the quality of the steel must, in the first place, be of such a nature that it will protect the consumer; that is, enable him to reject such steel as is not fitted for the purpose for which it is designed to use the metal. In the second place it must be practicable; that is, it must not require too elaborate appliances nor too much labor to make the test; and finally, it must not be so severe on the manufacturers as to

cause unneccessary hardship and loss in manufacturing the metal. What physical test now will fill these requirements? The data which have been obtained in the work done on the rails we have been studying enable us to prepare specifications which will make it possible to use any one of these four kinds of tests, viz., torsion test, tensile test, shearing test, or bending test. Thus we could, and if so desired will, prepare specifications leaving it optional with the steel-rail manufacturers which test shall be used. And I may be permitted to say here, that if the torsion test is chosen the data of the slower-wearing rails so strongly confirm the position taken in the first report to you on this subject that I do not see how anything else could be done than to re-recommend the specifications of the torsion test in that report.

But in looking over the physical tests of this series, I think it is plainly evident that the shearing tests and the bending tests bear a closer relation to the loss of metal per million tons than any of the other tests. It is true that there are some anomalous cases even in these tests; some cases that do not exactly fall into line. In the case of the bending tests this may in part, perhaps, be accounted for by the fact that these tests were made on samples cut from the web of the rail instead of from the head, where the wear took place. Mr. Cloud has found by a series of companion tests, made from samples cut from the head and web of the same rails, that the physical qualities of the metal in the head differ somewhat from that in the web, and, as has just been said, this may to a certain extent explain some of the anomalous cases in the series of bending tests. This closer agreement, however, between wear and the physical qualities of metals which are measured by shearing and bending, suggests that, perhaps, a shearing or bending test would be the best one to apply. And here I would like to express my judgement that the test suggested by M. J. T. Smith, General Manager of the Barrow Hematite Steel Works, several years ago, is, all things considered, the best physical test ever proposed for the examination of steel rails. Mr. Smith proposed that the fish-plate holes should be punched by a registering press, and that the quality of the steel should be judged by the pressure required to punch these holes. With the proper specifications as to the upper and lower limit of the punching strain, both steel that was too hard and steel that was worthless, from being spoiled in manufacture, could be rejected. And, still further, this punching test would be a shearing strain, and, at the same time, would enable a judgment to be formed

of the quality of the steel in every rail. A slight reaming of the holes after they were punched would remove the ring of metal injuriously affected by the punch, and leave the rails uninjured as to strength at the fish-plate holes. Unfortunately, however, with present appliances the punching test seems to be impracticable.

It would perhaps, therefore, be best to apply a bending test, especially if our analysis of the strain applied to the infinitesimal teeth of the rail in rolling friction is a correct one. It will be remembered that the theoretical considerations of what takes place in wear seems to plainly indicate that the strain applied to the minute elevations on the rail is a bending strain. If this is true, there could be no better test of the quality of steel to resist wear than a bending test.

Now there are four ways in which a bending test could be applied.

1st. The drop test. This test is in good use in England at the present time to determine the quality of steel for rails. But the disposition in this country, so far as my knowledge goes, seems to be to regard it as a somewhat crude test, and it is, therefore, being laid aside in favor of more accurate modes of testing.

2d. A piece of steel hammered out from a crop-end could be bent under a steam-hammer. This test was in use on the Pennsylvania railroad for some time to determine the quality of the steel, but its crudity, and the fact that the metal was manipulated after it had been rolled, and thus did not represent the steel in the rails, seem to me very justly to have led to its abandonment.

3d. A crop-end or piece of the rail just as it was rolled may be bent in a testing-machine. With proper specifications this test would undoubtedly give entirely satisfactory results. With a proper upper limit as to the maximum load required to bend the rail, steel that was too hard could be rejected, and, with proper specifications as to the amount of deflection the steel should stand before rupture, steel that was worthless from being spoiled in manufacture could likewise be rejected, and thus only good material be received. We would be glad to prepare specifications for this test if desired.

4th. A test-piece cut from the rail could be bent in the testing-machine. This test would undoubtedly likewise give us entirely satisfactory results. And for this test we have the data already at hand from which to prepare specifications. One or two things, however, should be borne in mind: 1st. The tendency of the steel rail manufacturers in this country at the present time is to make steel much

harder than is desirable, if our conclusions as to the wearing quality of steel are correct. This tendency must of course be recognized in preparing the specifications. And, in my judgment, this tendency will best be met by assigning an upper limit to the maximum load in the bending test. 2d. While our desire is to secure softer steel for rails, it must be remembered that steel may be so manipulated in manufacture as to render it very inferior in quality. Our specifications must therefore enable us to reject such worthless steel. This, in my judgment, will be successfully accomplished by requiring the test-pieces to bend to a certain angle before rupture.

I would, therefore, recommend that on all steel for rails received by the Pennsylvania Railroad Company in future the following bending test be prescribed: The test-pieces shall be cut from the web of the rail and shall be 12 inches long, 1½ inch wide, and ½ inch thick. These pieces shall be tested in the manner prescribed in the preceding part of this report, and on all steel accepted these test-pieces shall stand a maximum load of not over 3,000 pounds, and shall bend not less than 130° without rupture.

It gives me pleasure to make acknowledgments for services rendered to those who have assisted in this investigation. Mr. J. W. Cloud, engineer of tests, has had charge of the physical tests and measurements, and has either made them himself or had them made under his supervision. He has also from time to time made suggestions and contributed to the development of the ideas that have been worked out during the investigation in so many instances that it would be difficult to enumerate them. Mr. A. O. Dayton, assistant engineer of tests, made all the measurements with the planimeter, and likewise assisted in making the physical tests. Mr. H. L. Wells, at present chemist of the Colorado Iron and Coal Company, Pueblo, Colorado, did quite a portion of the chemical work. The phosphorus and manganese were principally determined by him. The silicon was determined by Mr. Wells and myself working together. The carbon was determined by myself. The services of my assistant, Mr. Theo. J. Lewis, have been almost invaluable in every part of the investigation. His patience, industry and careful attention to details can only be appreciated by those who know how laborious has been the work of collecting and working out into its present form the information herewith presented.

Very truly yours,

CHARLES B. DUDLEY, *Chemist.*

EXPERIMENTS ON THE STRENGTH AND STIFFNESS OF SMALL SPRUCE BEAMS.

By F. E. KIDDER, B.C.E.

Read at the American Academy of Arts and Sciences, by Prof. Chas. R. Cross, S.B., February 9, 1881.

The following experiments were made in the physical laboratory of the Massachusetts Institute of Technology, being a part of the fourth year work in the course in architecture.

The object of the experiments was to determine the moduli of elasticity and of rupture in small beams of white sprace (*Abies alba*), and such other information as might be derived from the data obtained.

The machine used for the purpose consists of two solid wooden frames, carefully leveled and placed 40 inches apart. Upon the top of each frame is placed a movable plate of iron, which is carefully adjusted, so that the two plates shall be directly opposite each other, and exactly 40 inches apart between the faces. These plates form the supports for the beams.

The loads were applied by means of a scale-pan suspended from a $\frac{3}{4}$-inch bolt, which rested upon the centre of the beam. By means of an iron strap suspended from a horizontal beam placed above the test piece, and resting on two screws, the bolt from which the load was suspended, could be raised from or lowered upon the test piece as easily and gradually as could be desired.

The deflections of the beams were measured by means of a micrometer screw reading to $\frac{25}{10000}$ of a millimetre, or $\frac{1}{10000}$ of an inch. As the bolt from which the load was suspended rested on the centre of the beam, it was necessary to measure the deflections at a distance of one inch from the centre, but the deflections used in calculating the values of the modulus of elasticity were corrected, so as to give the deflection at the centre, supposing the curve assumed by the beam to be the arc of a circle, from which, in fact, it deviates but little under such small loads. In reading the micrometer, the principle of electrical contact was taken advantage of.

The chances for error in using the machine are as follows:

In measuring the deflections $\frac{1}{10000}$ of an inch. In the breaking load, possibly 1 pound; but in the small loads there could be no appreciable error. In measuring the dimensions of the test pieces $\frac{2}{1000}$ of an inch.

The experiments were conducted with the utmost care, and every possible precaution taken to prevent errors.

In arranging for the experiments, and while making them, the writer was greatly assisted by Mr. Holman, of the Massachusetts Institute of Technology, to whom he extends hearty thanks.

TABLE I.

No. of test piece.	Clear span. *l.*	Breadth. *B.*	Depth. *D.*	*E*	E_1	*R*	Centre breaking w't for beam. 1″×1″. *A.*
	Ins.	Ins.	Ins.	Lbs.	Lbs.	Lbs.	Lbs.
1	40	1·475	1·45	1,731,000	1,657,000	11,380	632
2	40	1·445	1·52	1,556,000	1,528,000	10,330	574
3	40	1·469	1·448	1,765,000	1,732,000	10,710	595
4	40	1·42	1·498	1,736,000	1,636,000	10,830	601
5	40	1·45	1·485	1,688,000	1,578,000	11,980	665
6	40	1 48	1·44	1,795,000	1,686,000	11,040	613
7	40	1·464	1·46	1,682,000	1,561,000	10,570	587
8	40	1·42	1·48	1,647,000	1,556,000	11,280	626
9	40	1·46	1·46	1,704,000	1,638,000	11,180	621
10	40	1·441	1·46	1,616,000	1,550,000	12,440	691

Average value of *E*, 1,692,000 lbs.; of E_1, 1,612,000 lbs.
" " *R*, 12,170 lbs.. of *A*, 620 lbs.
Weight of cubic foot, 25·7 lbs.

The pieces of wood experimented on were sawn from a spruce plank that had been cut in Eastern Maine in the spring of 1880, and the following summer shipped to Boston, where it had lain in the open air until it was cut up in October. The pieces were carefully planed to an approximate size of 1½ inches square, and 4 feet long.

They were nearly all straight grained, and had but few defects, and

in testing the beams they were placed so that the defects should have the least possible effect upon the strength of the beams.

The exact dimensions of the test pieces are given in Table I.

In making the experiments, each beam was first subjected to a load of 30 pounds and the deflection noted. The weight was then left on the beam for a period of time varying from 1 to 4 hours, and in one case 44 hours, and the deflection again noted. The load was then removed from the beam, and the set noted, after which the beam was allowed a certain time to recover from the set.

After the piece had returned, or at least nearly returned, to its original position, it was subjected to a load of 40 pounds in the same manner.

Table II gives the deflections of each piece under the loads of 30 and 40 pounds, both immediately after the weight was applied, and after it had rested upon the beam the length of time designated.

The value of the modulus of elasticity, calculated from these deflections, is also given.

Modulii of elasticity, obtained from the deflection of the beams immediately after the weight was applied, have been denoted by E, and those obtained from the deflection of the pieces, after the weight had been applied one or more hours, by E_1.

Table I gives the values of E and E_1, for each piece, obtained by taking the average of the values given in Table II.

The values of E were computed by the formula $E = \frac{W l^3}{4 \Delta B D^3}$, in which W denotes the weight in pounds producing the deflection; l the clear span in inches; Δ the deflection of the beam at the centre; B the breadth of the beam, and D the depth, both in inches.

After all the beams had been tested in this way, piece No. 3 was again put in the machine and subjected to a load of 100 pounds, which was allowed to remain upon the beam for about two hours, the deflection being measured directly after the weight was applied, and just before it was removed. The beam was then allowed a certain time to recover its set. In two cases the beams, after having been subjected to a load of 100 pounds, finally returned to their original position, and it appeared probable that all would have done so had sufficient time been allowed for the purpose.

After the piece had nearly recovered from the effects of the load of

100 pounds, a load of 150 pounds was put on the beam, and gradually increased until the breaking point was reached.

The remaining pieces were tested with a load of 100 pounds in the same way, and then subjected to a load of 400 pounds for one or two minutes, for the purpose of getting the deflection under that load, and immediately after subjected to the full load of 500 pounds, which was gradually increased until the piece broke. As the load approached the breaking weight, it was increased by the addition of only one or two pounds at a time, so that the breaking weight could be obtained with sufficient accuracy. In fact, the breaking weight is so much modified by the time occupied in breaking the beam that it is difficult to ascertain exactly what it really is. For any load, over three-fourths of what is called the breaking weight, would probably break the beam if applied for a sufficient length of time.

Table I gives the values of the modulus of rupture of each piece, computed by the formula $R = \frac{3}{2} \cdot \frac{Wl}{BD^2}$, in which R denotes the modulus of rupture; W the breaking weight of the beam, and the other letters have the same significance as in the formula for E. The load which would break a beam of the same wood, one inch square, and one foot between supports, if applied at the centre, is also given in the same table. This load is $\frac{1}{18}$ of the modulus of rupture.

When the weight of 400 pounds was applied to piece No. 7, it immediately cracked at a gnarl in one of the lower edges, about three-fourths of an inch from the centre of the beam. As it was thought that the beam would soon break entirely, the load of 400 pounds was allowed to remain on the beam; but at the end of 100 hours the deflection had only increased 0·2224 inch, and, as it was evident that it would at that rate take a long time for the beam to break, the load was then gradually increased until the piece broke at 550 pounds, giving a modulus of rupture considerably above the average. It was noticed in this beam that the deflections under loads above 500 pounds were considerably greater than in the other beams under the same loads.

Piece No. 5 gave a very high breaking weight, and broke very suddenly, more like the harder kinds of wood. The fracture was very perfect; the upper half of the fibres being very evidently compressed, and the lower half suddenly pulled apart, with almost no splintering. This piece had a small knot on the upper side, 5 inches from the centre

of the beam, but it appeared to have no effect upon the strength of the beam.

TABLE II.

No. of test piece.	Weight. W.	Time applied.	Deflection. Δ.	E	Weight. W.	Time applied.	Deflection. Δ	E
	Lbs.	H. M.	Ins.	Lbs	Lbs.	H. M.	Ins.	Lbs.
1	30	0 00	0·0610	1,744,000	40	0 00	0·0826	1,719,000
1	30	2 25	·0630	1,689,000	40	3 30	·0873	1,627,000
2	30	0 00	·0616	1,536,000	40	0 00	·0801	1,576,000
2	30	1 45	·0639	1,481,000	40	3 00	·0842	1,499,000
3	30	0 00	·0606	1,774,000	40	0 00	·0815	1,757,000
3	30	1 00	·0610	1,759,000	40	1 00	·0840	1,705,000
4	30	0 00	·0582	1,725,000	40	0 00	·0765	1,748,000
4	30	2 25	·0616	1,627,000	40	3 20	·0813	1,645,000
5	30	0 00	·0580	1,740,000	40	0 00	·0774	1,637,000
5	30	2 30	·0618	1,630,000	40	44 15	·0874	1,527,000
6	30	0 00	·0605	1,792,000	40	0 00	·0862	1,676,000
6	30	3 00	·0639	1,697,000	40	5 12	·0803	1,799,000
7	30	0 00	·0624	1,683,000	40	0 00	·0833	1,682,000
7	30	3 00	·0663	1,584,000	40	16 00	·0911	1,538,000
8	30	0 00	·0632	1,645,000	40	0 00	·0839	1,650,000
8	30	4 15	·0666	1,561,000	40	16 30	·0894	1,552,000
9	30	0 00	·0619	1,701,000	40	0 00	·0823	1,707,000
9	30	1 30	·0643	1,638,000				
10	30	0 00	·0661	1,614,000	40	0 00	·0881	1,618,000
10	30	2 00	·0691	1,544,000	40	1 15	·0915	1,556,000

Piece No. 4 broke in a rather peculiar manner. While under a load of 575 pounds the lower fibres, for about a depth of one-tenth of an inch, snapped apart, and the beam gradually settled down until the next layer of fibres had apparently the same deflection as had the lower

ones at the time of breaking, when they also snapped, making a layer of about the same thickness. In this way the whole lower half of the beam seemed to divide itself into layers of about one-tenth of an inch thick, and to break separately, under about the same deflection, so that the beam was a long time in breaking.

Observing that under every load that had been applied the deflection kept increasing with the length of time the weight remained on the beam, piece No. 7 was subjected to a load of 275 lbs. for 98 hours, during which time the deflection increased 0·079 in. The weight was then taken off, and the beam allowed to recover for 24 hours, when it had a set of ·0446 in. The same weight was again applied, and it was found that the deflection, obtained by taking the difference between the readings just before and after the weight was applied, was less than it was the first time that the weight was applied, and the rate of increase of the deflection was about the same as before. The beam was thus subjected to a load of 275 lbs. for 300 hours in all, and it was then broken in the same manner as the others. It was expected that the effect of such a severe strain on the beam for so long a time would diminish the strength; but, on the contrary, it appeared to increase it, as the beam gave a higher modulus of rupture than any of the others, although it did not appear to be of as good quality as many of them. The ultimate deflection of this beam greatly exceeded any of the others.

Table III shows the deflection of each beam under loads of 30, 40, 100, 400, 500 and 550 lbs. immediately after the load was applied and at a distance of one inch from the centre. The *italic figures* under each deflection show what it would be if Hooke's law held true, taking the deflection under 30 lbs. as the starting point.

From these experiments I think we may draw the following conclusions:

That the modulus of elasticity depends not only upon the elasticity of the material, but also upon the length of time that the load is applied.

That when subjected to loads not exceeding one-sixth of the breaking weight spruce beams do not take a permanent set.

That even under very small loads, if applied for any length of time, there will be a temporary set.

That knots and gnarls in beams loaded at the centre, when not

within one-eighth of the span of the centre of the beam, do not materially affect the elasticity under small loads.

TABLE III.

No. of test piece	Deflections in Inches, under						Ultimate deflection, in inches.	Breaking Weight.
	30 lbs.	40 lbs.	100 lbs.	400 lbs.	500 lbs.	550 lbs.		
	Ins.	Ins.	Ins.	Ins.	Ins.	Ins.		Lbs.
1	0·0610	0·0826	0·2071	0·8303	1·0906	1·2791	1·5646	588
	·0610	*·0813*	*·2030*	*·8120*	*1·0150*	*1·1165*	.....	.....
2	·0616	·0801	·2043	·8177	1·0677	1·2811	1·3941	575
	·0616	*·0820*	*·2050*	*·8200*	*1·0250*	*1·1275*	.....	.....
3	·0606	·0815	·2023	·8976	1·2764	1·48*	1·48*	550
	·0606	*·0808*	*·2020*	*·8080*	*1·0100*	*1.1111*	.....	.....
4	·0582	·0765	·1929	·7929	1·1146	1·3197	1·4658	575
	·0582	*·0776*	*·1940*	*·7760*	*·9700*	*1·0670*	.....	.....
5	·0580	·0774	·2004	·7876	1·0170	1·1827	1·5788	637
	·0580	*·0773*	*·1933*	*·7732*	*·9665*	*1·0631*	.....	.....
6	·0605	·0803	·2138	.....	1·2520	1·4662	.....	565
	·0605	*·0806*	*·2016*	.....	*1·0080*	*1·1088*	.....	.....
7	·0624	·0833	·2083	·8961	1·3595	.....	.....	550
	·0624	*·0832*	*.2080*	*.8320*	*1.0400*	.....	.....	.....
8	·0632	·0839	·2102	·8315	1·1111	1.3331	1·5709	585
	·0632	*·0842*	*·2106*	*·8424*	*1·0530*	*1·1583*	.....	.....
9	·0619	·0823	·2083	.....	1·0800	1·2830	1·4254	580
	·0619	*·0825*	*·2063*	.....	*1·0315*	*1·1346*	.....	.....
10	·0661	·0884	·2220	·9276	1.1772	1·3775	1·81*	637
	·0661	*·0881*	*·2203*	*·8812*	*1·1015*	*1·2116*	.....	.....

That the deflection is very nearly proportional to the load, far beyond the customary limits of strain, and that the modulus is consequently very nearly constant for all moderate deflections.

* Approximately.

That a high modulus of elasticity does not always accompany high transverse strength. For, as shown by Table I, piece No. 10, which had the greatest transverse strength, gave next to the lowest value of E.

That in spruce beams the upper fibres commence to rupture by compression under about four-fifths of the breaking weight, and the neutral axis, at the time of rupture, is very near the centre of the beam, as shown by the fracture.

That beams which are subjected to severe strains. for a long time, bend more before breaking than those which are broken in a comparatively short time.

That the modulus of elasticity of small spruce beams, of a quality such as is used in the best buildings, may be taken at from 1,600,000 to 1,700,000 lbs., and the modulus of rupture at 11,000 lbs.

The only other experiments on American spruce with which the writer is familiar are those made by Mr. Hatfield, on small beams, 1·6 feet between supports, and some experiments by Mr. Thomas Laslett, of England, on pieces of Canada spruce, 2 inches square and 72 inches between supports.

Mr. Hatfield gives as the average value of the transverse strength of a unit beam 612 lbs.,* which would give 11,016 lbs. for the modulus of rupture.

From data given by Laslett† we obtain as the value of R, 9045 lbs.

The value generally given for the modulus of elasticity of spruce is 1,600,000 lbs.

Length of the Second's Pendulum.—C. S. Peirce has re-examined the estimates of the length of the seconds' pendulum at Paris, by Borda, Biot and Kater. He finds that if allowance is made for the inertia of the air which is dragged by the friction of the pendulum, the estimated length should be increased about $\frac{1}{100}$ of 1 per cent. He therefore regards the true length as 993·934 millimetres (39·132 in.) Faye compliments Peirce's investigation very highly, and he congratulates the French Academy upon the great accuracy of the previous experiments, which were conducted largely under its auspices, when the facilities for minute measurements were not so great as they are now.—*Comptes Rendus.* C.

* Hatfield's "Transverse Strains," Table XLII.

† "Timber and Timber Trees, Native and Foreign," by Thomas Laslett. Inspector to the Admiralty. London, 1875.

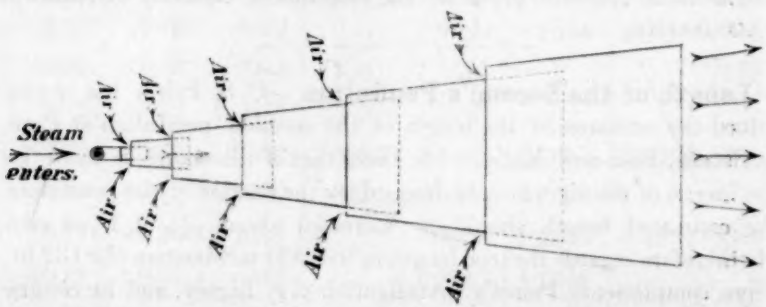
EXPERIMENTS MADE BY MR. SCHEURER-KESTNER WITH THE KŒRTING APPARATUS FOR THE INSUFFLATION OF AIR BY MEANS OF A STEAM-JET.

Translated* from the Bulletin of the Industrial Society of Mulhouse for the year 1878, page 674, by

Chief-Engineer ISHERWOOD, U. S. Navy.

The construction of Mr. Kœrting's apparatus is based on the principles employed in the Giffard injector and similar contrivances, with the single difference that in the latter a gas or vapor and a liquid are the reacting substances, while in the former they are gases or vapors. The Kœrting apparatus is used in various industries either for drawing in gases and then discharging them against pressure, or for mingling them with liquids; we have used it during two years in the factory of Thann for various purposes.

The necessity under which I found myself of knowing exactly the economic performance of this apparatus, that is to say, the effect produced and the expenditure of steam to produce it, caused me to make some experiments whose results seem worthy the attention of my colleagues of the Industrial Society.



The Kœrting apparatus (see sketch) is composed, abstractly, of a hollow right cone, from which frustra at intervals had been removed, and the remaining frustra, with heights proportional to their diameters, telescoped together in such manner that each is partly inserted into the next largest, an annular space being left between. This system is

*Some additions have been made to the author's description of the apparatus, and his calculations have been corrected for a known error. TRANSLATOR.

placed concentrically within an open-ended cylinder. High pressure steam by means of a small pipe is delivered at large into the smallest end of the smallest frustrum, traversing in succession the succeeding frustra, thereby drawing in through the annular spaces separating them the gas or air between them and their enveloping cylinder, the mixed steam and air being finally discharged from the base of the largest frustrum.

My experiments were made on a No. 2 Kœrting apparatus. The steam expended was ascertained by measuring directly the water vaporized in the boiler exclusively employed for furnishing it, and in which the water level and steam pressure were maintained strictly constant. The air drawn in was measured by an anemometer, the instrument I had used for several years to measure the air drawn in by coal burning and by sulphur furnaces. For this purpose a tube of 0·65618 foot diameter and 3·2809 feet length was placed at the receiving end of the cylindrical envelope into which one-half the length of the tube was inserted.

To verify, at least approximately, the performance of the apparatus I made some preliminary trials, in which I compared the relative velocity of the blades of the anemometer corresponding to observed variations of depression in the cylindrical envelope. The volume of air flowing through the latter, calculated from the number of revolutions made by the blades of the anemometer in a unit of time, agreed almost exactly with what could be deduced from the differences of depressions. The proper coefficient for the instrument being thus ascertained, I employed it in the following experiments.

The anemometer, made by Mr. Neumann of Paris, was numbered 225, and the formula for calculating the volume V of air is

$$V = 0{\cdot}590562 \text{ foot} + \overline{0{\cdot}5675957 \text{ foot}} \times n,$$

n indicating the number of revolutions made per second by the blades of the instrument.

The drawn in gas was discharged under a column of water 78¾ inches high into a wooden tank of 43⅓ inches diameter.

Steam pressure in the boiler, 57 pounds per square inch above the atmosphere.

Weight of water vaporized, 707·6834 pounds.

Duration of the experiment, 81 minutes.

Depression, 1·96855 inches of water.

Temperature of the gaseous mixture (air and steam), 185° to 186·8° Fahrenheit.

Temperature of the air, 77° Fahrenheit.

The temperature of the liquid did not exceed 186·6° Fahrenheit. The weight of steam condensed, determined from the increased volume of the water, was 224·8714 pounds; that is to say, about two-thirds of the steam were disengaged from the liquid, together with the overplus of air.

During the above time the axis of the blades of the anemometer made 95,561 revolutions, or 19·25123 per second.

Applying the foregoing formula, we find the velocity of the air in the adjutage to be $V = 0{\cdot}590562 + \sqrt{0{\cdot}5675957 \times 19{\cdot}25123} = 11{\cdot}517477$ feet per second. As the experiment lasted 81 minutes, the length of the gaseous cylinder drawn in was

(81 minutes × 60 seconds × 11·517477 feet =) 55974·9382 feet,

the diameter of the adjutage being 0·65618 foot, the corresponding area to which is 0·3402212 square foot, the gaseous volume drawn in was 19043·8606 cubic feet.

Summing up the preceding, we have obtained against a resistance of 78¾ inches of water a gaseous volume of 19043·8606 cubic feet by an expenditure of 707·6834 pounds of steam, of which 224·8714 pounds were condensed.

As one pound weight of steam of the temperature of 186° Fahrenheit under the standard atmospheric pressure occupies 24·9877 cubic feet, we have for the centessimal composition of the volume of mixed air and steam discharged from the Kœrting apparatus

Air, ...	51·852
Steam, ...	48·148*
	100·000

The greater the pressure of the boiler steam employed the less will

* The weight of a cubic foot of air at the temperature of 77° Fahrenheit, under the standard atmospheric pressure, being 0·0739359 pound, the weight of the air entrained by the 707·6834 pounds of steam was 1408·0250 pounds, from which we find that the gaseous mass of mingled air and steam discharged from the Kœrting apparatus consisted by weight centessimally of

Air, ...	66·55
Steam, ...	33.45
	100·00

TRANSLATOR.

be its expenditure in the Kœrting apparatus; in other words, the more the steam is expanded and its density decreased, the more will its useful effect be increased. This is what theory predicts and practice confirms. In another experiment, where the steam pressure was maintained in the boiler at 71 pounds per square inch above the atmosphere, instead of 57 pounds, the weight of steam expended for the same volume of air drawn in fell from 524·21 pounds to 445·33 pounds per hour, a gain of 17·713 per centum in favor of the higher pressure.

When the current of mixed air and steam is discharged at large into the open atmosphere, and has no pressure to overcome, the weight of steam necessary to produce a given air entrainment diminishes in a very high ratio. A gaseous mixture of air and steam can then be obtained whose temperature will not exceed that of the atmosphere more than from 18° to 22° Fahrenheit.

The following experiment was made with another Kœrting apparatus not working against pressure, the gaseous mixture being discharged at large into the open air.

Steam pressure in the boiler, 71 pounds per square inch above the atmosphere.

Weight of water vaporized, 1102·3106 pounds.

Duration of the experiment, 7 hours.

Temperature of the gaseous mixture (air and steam), 88° Fahrenheit.

Temperature of the atmosphere, 72° Fahrenheit.

The number of revolutions made by the axis of the blades of the anemometer was 12 per second.

$$V = 0{\cdot}590562 + 0{\cdot}5675957 \times 12 = 7{\cdot}4017104 \text{ feet per second.}$$

The velocity of the entrained air being 7·4017104 feet per second, and the duration of the experiment 420 minutes, the length of the gaseous cylinder drawn in is

(420 minutes × 60 seconds × 7·4017104 feet =) 186523·1021 feet.

The cross area of the adjutage being 1·3552253 square feet, the total volume of air drawn in is

$$186523{\cdot}1221 \times 1{\cdot}3552253 = 252780.8270 \text{ cubic feet.}$$

During the same time the weight of steam expended was 1102·3106 pounds, representing in volume (21·0059 cubic feet per pound) 23155·0262 cubic feet. The discharged gaseous mixture is then composed of 252780·8270 cubic feet of air and of 23155·0262 cubic feet of steam, or by volume centesimally:

Air,	91·6086
Steam,	8·3914*
	100·0000

These experiments show the extent to which the expenditure of steam can be reduced; and it is evident that by operating under other conditions, that is to say, with a greater expansion of the steam, the expenditure can be still more considerably reduced.

Novel Volcanic Phenomenon. — Within the space of ten months Mt. Etna had five abundant eruptions of smoke and sand, without any subsequent flow of lava. In one instance, after profound subterranean rumblings and numerous earthquake shocks, there appeared on the eastern side of the mountain a great cloud of vapors and ashes, which escaped by a crevice nearly three miles long. The snows melted suddenly around the summit of the mountain, jets of hot vapor escaped at many places, and the small muddy craters of the western declivity became very active, as is usually the case on the approach of a great eruption. But to the surprise of all observers, within thirty-six hours afterwards the volcano had returned to a state of perfect calm. Such a phenomenon has never before occurred within the memory of man. Vicenzo Tedeschi di Ercole attributes it to the existence of an immense opening, which appeared upon the mountain at the time of the eruption of May 26th, 1879. He concludes that a very strong pressure is required for the formation of lava, and that a great tension of gas is indispensable in order to raise the lava to the surface of a mountain. It appears probable, therefore, that there will be no reason to fear any further eruption in the cone of Etna as long as the present crevice is open.—*Ann. de Chim. et de Phys.* C.

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* The weight of a cubic foot of air at the temperature of 72° Fahrenheit, under the standard atmospheric pressure, being 0·0746120 pound. the weight of air entrained by the 1102·3106 pounds of steam was 18860·4831 pounds, from which we find that the gaseous mass of mingled air and steam discharged from the Kœrting apparatus consisted by weight centesimally of

Air,	94·48
Steam,	5·52
	100·00

TRANSLATOR.

OBSERVATIONS ON THE WATER SUPPLY OF PHILADELPHIA.

By Reuben Haines.

It is well known that adverse criticism has of late years frequently been made on the character of the water supply of Philadelphia, both by residents and by visitors from other cities. To residents of Philadelphia it has not been satisfactory for a number of years for several reasons, such as shortness of supply during long summer droughts and consequent risk in case of fire; secondly, the frequent muddiness of the water so as to render it at times quite unfit for household use without domestic filtration, entailing some additional expense and trouble, which induces many to use well water in preference; and thirdly, well-grounded fears that the Schuylkill and Delaware river waters are, year by year, increasingly assuming a character which will eventually prove dangerously unwholesome, by reason of contamination with human sewage and factory refuse. It is admitted that a large amount of sewage passes into the Schuylkill above the several water-works, a fact capable of ocular demonstration; but it cannot be proved that any disease arises at present from that source, and the low death-rate of Philadelphia to a certain extent shows that its health is not materially affected by a contaminated water supply, especially when we consider the fact that many of the deaths are caused by blood-poisoning by sewer air through defective plumbing. Many people rely on the theory that most of the organic matter of sewage is destroyed during the flow of a few miles in the river, and since the Schuylkill water has never appeared to be unwholesome, they are naturally reluctant to approve of the expense of any change in the source of supply on this account.

But will this present wholesomeness continue? Is it true that the sewage of the Schuylkill is actually destroyed during the river-flow; and if so, to what extent does this destruction by oxidation and other means take place? Assuming that this oxidation may occur, may not a something causing infectious disease be liable to accompany this sewage, which will resist all destructive influences; and therefore, may not the mere fact of sewage contamination be a danger signal for

the future? These are questions of vital public interest; yet no effort has been made by Philadelphia for a practical solution of them, as regards the Schuylkill river, and no official examination has ever been made by this city of the experiments conducted in other places, concerning which various conflicting opinions have been expressed by different authorities.

A number of analyses of the Schuylkill water have been made by several chemists at different times, and the samples have been taken at various points. Enough has been given by Dr. Cresson in his report to the Chief Engineer of the Water Department, March 3d, 1875, to make it appear that the condition of the water is very variable, and that these variations do not correspond at all closely with the seasons. Sometimes the pollution is greatest during the summer months, while sometimes it is then at a minimum. The same may be said of the winter months. It is shown also, that the purity of the supply varies according to the location of the different pumping-stations.

From Dr. Cresson's analyses in the paper cited above, I have deduced the following averages, which I have calculated in parts by weight of ammonia in one million parts of water, the metrical form of statement being superior to all others for purposes of comparison:

	Free Ammonia.	Albuminoid Ammonia.
I. Average composition of water in Fairmount forebay from Feb., 1872, to Feb., 1875, by fourteen analyses in different months, . .	0·09	0·236
II. Average composition of the same from Feb., 1872, to Jan., 1873, by eight analyses in eight different months, ...	0·05	0·156
III. Average of same, from Jan, 1873, to Jan., 1875, six analyses, ...	0·12	0·447
IV. Average of same, from Jan., 1874, to Jan., 1875, four analyses, ...	0·12	0·541

From these results, considered by themselves, it might seem that the statement made by Dr. Cresson to the effect that the amount of sewage has been steadily increasing since 1872, until the water is occasionally charged with an amount of sewage exceeding that carried by the Thames at London is fully justified.

This is certainly an astonishing result considering that this water has been heretofore considered to be of standard good quality. A large

amount of sewage which passed into the river shortly previous to 1875 has since been diverted by a sewer on the west bank of the river. If there had been a sudden increase in this pollution it might partially explain the results of the analyses.

These results are, however, strangely in contrast to the analyses of the Fairmount water, made in 1875 by Booth and Garrett and published in their report for the Water Commission of that year, which gave in parts per million—free ammonia, 0·02; albuminoid ammonia, 0·03. This sample of water was, as they rightly remark, equal in purity to the water from the "artesian" well at Bryn Mawr, and was perhaps even somewhat better than the water from an "artesian" well in Germantown, 300 feet in depth, which I analyzed in August, 1877. It is to be regretted, however, that Messrs. Booth and Garrett give no information whatever as to the exact dates of their analyses, the condition of the weather previous to the collection of the sample, height of river, etc.; nor do they state whether their results are single analyses or averages of several samples; nor, again, the exact place where the sample was collected. These circumstances are so important for the proper comparison of analyses that much of the value of their results is thus destroyed.

It should also be said, that while Dr. Cresson does give some of these particulars, there are others he has not given.

I have made only a very few analyses of the Fairmount water, nevertheless the results may be of some interest. A sample was taken March 1, 1878, from a water-cock in a store at Seventh and Commerce streets, and therefore, represented the water as actually delivered to consumers. It gave in parts per million—free ammonia, 0·014; albuminoid ammonia, 0·056. This result agrees tolerably well with that obtained by Booth and Garrett. I never placed much reliance, however, on the results of a single analysis because *a priori* considerations would lead us to expect considerable variations at different times and seasons. By several analyses made during the present winter, of water collected at No. 315 Willing's Alley, I am lead to believe that the organic matter is often about four times as great as that indicated in my analysis just stated. It is possible, however, that these results may be invalidated if it is true that the water flowing through the city mains consisted of the mingled waters from several sources poured into the same reservoir before distribution.

Were the results given by Booth and Garrett correct as a yearly ave-

rage, their remarks upon the excellent quality of the Fairmount water in their report cited above would be fully justified. But while I have not the slightest doubt as to the correctness of any single analysis by these distinguished chemists, unfortunately I fail to find in their report any evidence that their analyses correctly represent the average quality of the river water, nor do they make any such statement, although their remarks would appear to be based upon this inference.

It may seem to some persons hardly worth while to criticise the report of the Water Commission after a period of five years has elapsed since its publication. But this objection may be disposed of by the consideration that almost nothing has been done toward adopting the recommendations of that report, or indeed, nothing at all to improve the purity of the Schuylkill water, except the construction of the sewer on the west bank of the river. Hence, the whole subject may be considered to be still in suspense.

Moreover, the City Councils have been repeatedly urged to carry into effect the suggestion made in that report to take water for the city supply from the Roxborough water works at Flat Rock dam, on the assumption that the water at this point was purer than the water at Fairmount, because it was taken from the river above the city limits.

Booth and Garrett's analyses, however, make it appear that in 1875. the Fairmount water was purer than that supplied by any of the other pumping works. If it had been stated that the samples were collected on the same day and under precisely similar circumstances, the evidence for this would have been much more conclusive.

My own analyses, although too few to form the basis of any definite conclusions, tend to confirm the impression drawn from those of Booth and Garrett. The amount of pollution was found to be less than the average condition during the whole year, 1872, by Dr. Cresson's analyses. My analysis of March 1st, 1878, given above, represents the state of purity frequently found as regards the water in the service pipes. An analysis of the Germantown supply from the Roxborough works, the sample being collected from the service pipe on the same day as the sample of Fairmount water above referred to, showed that the Germantown water contained twice the amount of organic impurity found in the water from Fairmount.

My analyses of the Germantown water have given the following

results, which are stated in parts per million, and all the samples taken from service pipes:

Date.	Place of Collection of Sample.	Free Ammonia.	Albuminoid Ammonia
June 18, 1877,	Corner of Main st. and Coulter st.,	0·127	0·091
" 22, "	" " "	0·034	0·124
" 23, "	" " "	0·032	0·116
July 13, "	" " "	0·034	0·140
" 13, "	Cor. Main st. and W. Walnut lane,	0·026	0·143
Feb. 4, 1878,	" " Coulter st.,	0·014	0·088
March 1, "	" " "	0·014	0·090
Nov. 21, 1879,	Wayne st. below Queen st.,	0·016	0·096
Dec. 7, 1880,	" " "	0·034	0·100
" 8, "	" " "	0·054	0·126
" 10, "	" " "	0·074	0·180
Jan. 11, 1881,	" " "	0·186	0·096
	Average of the above,	0·054	0·116

The following are a few analyses I have made of the city water, the samples also being drawn from the service pipes. Analyses stated in parts per million:

Mar. 1, 1878,	Seventh and Commerce streets,	0·014	0·056
Nov. 26, "	No. 315 Willing's alley,	0·030	0·120
" 27, "	" "	0·054	0·120
" 27, "	No. 1126 Ridge avenue,	0·066	0·200
" 27, "	Duplicate analysis of same sample,	0·074	0·206

It is thought it would be interesting to compare with these results analyses of the water of the Wissahickon and Wingohocken creeks:

Date.	Description	Free NH_3	Alb. NH_3
May 29, 1880,	Wissahickon creek, short distance above Norristown R.R. bridge,	0·240	0·290
July 14, 1877,	Wingohocken creek, East Branch, just above J. S. Haines' dam,	0·340	0·136
Aug. 1, 1877,	Wingohocken creek, West Branch, at East Walnut lane bridge,	0·112	0·192

The surface water was carefully excluded in taking these samples. The chlorine in each was respectively in the order given: —0·7, —0·85, and 1·15 parts in 100,000. The Wissahickon water was collected during a long continued drought and was quite clear and free from the mud usually found in it. The east branch of the Wingohocken received the drainage of a farm-yard above the place where the sample was taken. The west branch of this creek received the drainage

of pig styes and other surface pollutions. It was used for domestic purposes by a settlement of colored people.

We will now quote analyses by Booth and Garrett of the Schuylkill river waters given in their report of 1875, translating their figures into parts per million for the sake of comparison with the foregoing analyses.

	Free Ammonia.	Albuminoid Ammonia.
Fairmount,	0·020	0·030
Belmont,	0·100	0·087
Spring Garden,	0·299	0·149
Flat Rock (Roxborough works),	0·125	0·087
Perkiomen creek,	0·125	0·125
From Wanklyn's Water Analysis:		
Thames and River Lea water, supplied by London companies, average of 44 samples,	0·010	0·090
Thames water at Hampton court (London supply) before filtration, average of 2 samples,	0·025	0·255
Thames river at London bridge at high tide, average of 3 samples,	1·020	0·550

Dr. Cresson's analyses give the following as the average composition of the Schuylkill water at the Spring Garden, Belmont and Roxborough works:

Spring Garden, average of two analyses,	0·61	0·835
Belmont, average of five analyses,	0·45	0·454
Roxborough, average of two analyses,	0·37	0·327

From this discussion the following conclusions may, I think, be legitimately drawn.

That the analyses by Booth and Garrett and Dr. Cresson of the water in the river at the works, and my own analyses of the water as delivered in the service pipes, all coincide in showing that the average quality of the water contained in Flat Rock pool is not likely to be any better than that of the water pumped at the Fairmount works; provided we exclude from this calculation Dr. Cresson's analyses for the year 1874, when, even if the results were not very approximately correct, there must have been some enormous and temporary pollution of the river. The figures in the analyses of July 24th of that year are, however, so very extraordinary that they excite the most serious

doubts as to their correctness, and they therefore cast suspicion on several of the other analyses also. For, to make this more evident, it may be said that the albuminoid ammonia in the analysis of the Spring Garden water of that date is equivalent to that in the average of twenty-seven samples of actual sewage taken at night from the sewers of Worcester, Mass., during prevalence of dry weather.*

	Free NH_3.	Alb NH_3.
I. Average of 27 samples of sewage from three sewers in Worcester, Mass , taken at 6 A.M., 9 P.M. and 12 P.M. Night sewage, .	7·45	1·44
II. Average of 27 samples of the same taken at 9 A.M., 12 M. and 6 P.M. Day sewage, .	18·76	3·16
III. Dr. Cresson's analysis of the water at Spring Garden inlet, July 24, 1874, . .	0·29	1·45

These results are expressed in parts per million as heretofore in this paper

What is equally remarkable the water at Belmont inlet and Fairmount forebay were, according to Dr. Cresson's analyses, nearly as bad.

Bearing in mind that this albuminoid ammonia represents relatively the nitrogenous organic matter actually present both in solution and in suspension, Dr. Cresson's analyses appear altogether incredible. It will be impossible, we are sure, to make Philadelphians believe that at that time they were drinking liquid out of a veritable sewer. Moreover, his figures would represent the Fairmount water as twice, and the Spring Garden water nearly three times, as bad as the Thames river water at London bridge, which Wanklyn characterizes as "vile and stinking," having received a large part of the sewage of London.

For these reasons we must believe that some great mistake was made either in the mode of collecting the samples, concerning which the most scrupulous care is absolutely essential, or, in the mode of conducting the analysis, or in the calculation of the results.

In the comparison of the water from the different pumping stations, Dr. Cresson's analyses of November 7th, 1874, of the Fairmount and Roxborough waters collected on the same day, show a difference in favor of the former. Yet here again the amount of albuminoid ammonia in the Roxborough water is about five times as great as the amount found in the average of my own twelve analyses made in all

* *Vide* Fourth Report of Mass. State Board of Health, 1873,, page 79. Compare also analyses of Boston Sewage. Idem p. 70.

seasons, from 1877 to 1881. It is about four times as great as Dr. Cresson found it to be only seven months afterwards. We must therefore consider the correctness of these particular analyses also as very doubtful, so far as representing the true condition of that part of the water supply of this city. Variations in quality from day to day and from month to month will undoubtedly occur according to the variations from time to time in the amount and character of the waste material of all sorts from the numerous factories, and also according to the amount of rainfall and consequent flushing of stagnant sewers, etc. But that these variations should be of the extent indicated by Dr. Cresson's analyses is incredible, especially in comparison with the results of the examination of a considerable number of the rivers of Massachusetts by authority of the Board of Health of that State, and where the pollution is of a somewhat similar character.

A report has lately been made on the character of the Ohio river water at Cincinnati, used for city supply; but it appears that, although the variations are exceedingly great the deductions to be drawn from them cannot properly apply to the Schuylkill, because the conditions affecting the flow and pollution of these two rivers are so very different. During September and October of the past year the water was, perhaps, what might be called tolerably good, which is stated to represent about the average condition for the greater part of the year; but during the latter half of November, and especially during December, it became frightfully bad—actually nearly as bad, according to the same series of analyses, as the water was at about the same time at the mouth of a sewer.* One of the ways in which the Ohio river is polluted is the frequent practice of the railroad men on the cattle trains of throwing the carcases of animals, that have died on the way, into the river at the bridges. Large numbers of these dead bodies are sometimes seen floating down the river. Analyses of water from wells several hundred feet deep on the river "bottoms," or flood ground, show this water to be as bad as the river itself, and that this earth contains enormous quantities of organic matter deposited during the frequent floods.

To return to the condition of the Schuylkill river my analyses show that a considerable amount of pollution exists at certain times in the supply from the Roxborough works. Yet the results of these analyses may possibly be affected in some degree by vegetable growth in the

* *Vide* Report of Analyses of Ohio river water by order of the Board of Health, Cincinnati, Dec. 20, 1880. By Prof C R Stuntz.

reservoirs and distributing mains, which is known sometimes to occur. On reference to these analyses it will appear that the largest amount of albuminoid ammonia for 1877 was 0·14 parts per million, and for 1880, 0·18, while the average for all was 0·11 parts per million. On referring to the analyses by Prof. Nichols of the water of Cochituate Lake, the purest water supply of Boston, published in the fifth report (1874) of the Massachusetts State Board of Health, page 117, we find that the maximum amount of albuminoid ammonia during the summer, autumn and early winter of 1873, was 0·13 parts per million, with an average of 0·11 parts. The samples in this case also were taken from service pipes at the Mass. Inst. Technology. While the average is the same in the two cases, the maximum is greatest in the Roxborough supply. If it be said that the difference is very insignificant it may be replied, that just this apparently insignificant difference in albuminoid ammonia will in the case of shallow wells constitute the difference between a well water which is passably good and one which may be dangerously polluted. In river waters, however, as has been explained in a preceding paper on this subject, we should not judge quite so strictly as in the case of shallow wells.

We should also consider in this connection the fact, which we believe to be true, that the Cochituate water contains extract of peat, since the gathering grounds for the supply of the lake are of a peaty character, and consequently a part of the albuminoid ammonia probably represents extract of peat which is by most observers considered quite innocent when in not very excessive amount. It will thus appear that the sewage contamination of the Roxborough supply is, in all probability, quite perceptibly greater than the Boston supply, inasmuch as no beds of peat are, I believe, to be found on the banks of the Schuylkill and its tributaries.

There is some reason to believe that either the Schuylkill river water was purer in 1875 than it has been during the period of my analyses, and perhaps, better than it was for the two years, 1873 and 1874, or else that the water at all the different pumping stations was at the time of Booth and Garrett's analyses perceptibly better than its average condition for 1875.

It should be stated that the maximum pollution indicated in my analyses was coincident in time with extremely low water in the river which had continued for some time, causing a probable concentration of the sewage, and therefore, not necessarily indicating an increase in

the absolute amount of sewage poured into the river. We cannot regard the water at these maximum periods as being of satisfactory character, since at such times a disagreeable taste, and even an odor, is perceptible and sufficiently marked to cause some people in Germantown to prefer well waters for drinking purposes.

It is proper to state that my own analyses have been conducted throughout with very great care as regards perfect cleanliness of apparatus, etc., and purity of reagents and with strict adherence to the directions given by Wanklyn in the third and fourth editions of his manual. The samples were collected according to the directions of Wanklyn and Frankland, the absolute cleanliness of sample bottles being especially regarded.

One point of some importance, to which, it is believed, no allusion has been made in published reports, is the probability that the quality of the water in Fairmount forebay usually may be said to be an average of that of the whole stream opposite to that pumping station, while this cannot be said of the water at any of the other works. This is owing to the fact that frequently during more than half the year no water flows over the comb of Fairmount dam, and consequently the whole stream, exclusive of a very small proportion necessary for the canal locks, passes through the forebay of these works to drive the turbines and supply the pumps, the whole flow of the river being utilized for these purposes, with the exeeption of what leakage occurs.

In the case of the other works the supply is taken from along shore, obviously the most impure part of the stream, and the water is pumped by steam power. It is evident that the Roxborough supply is also different from that at Fairmount, for although the former works are situated above Flat Rock dam, the water supply is here also pumped entirely by steam power and the river water flows quite continuously over the comb of that dam and the current is not therefore diverted to any great extent.

Hence, supposing that no polluting material were poured into the river between Flat Rock and Fairmount, and also supposing that the sewage was not oxidized to any extent during this flow we should naturally expect the water supplied from the Fairmount works to be of purer quality than that supplied from any of the other pumping stations.

This consideration will at once suggest a plan of obtaining a better

supply at all the upper works by extending the inlet pipe out into the middle of the river stream.

Setting aside the question of a more abundant supply for this city as not being within my province, and *considering solely its purity*, a very obvious method of improving its character is presented in the plan of constructing large covered sewers *on both sides* of the river, into which it shall be made compulsory to discharge every kind of artificial drainage and house-sewage and all factory refuse. These sewers to be effective at all should extend from near Flat Rock to a point below Fairmount, and were it not for engineering difficulties it would be better to extend them above Flat Rock dam. In this way the special pollution of Fairmount pool would be prevented and opportunity would be given for the oxidation of the sewage of Norristown, Conshohocken and other towns above Philadelphia. If it be true that this oxidation does take place and these intercepting sewers having been constructed, it would be possible to furnish the city with a purer supply than at present by drawing nearly the whole of the water from near Fairmount dam. By going up the river for the supply we would then only meet the sewage of Norristown, etc., which has been less thoroughly oxidized.

It is true that since the valley of the Schuylkill is destined in the far future to be densely populated, this plan will necessarily be only a temporary one. For we must look forward to a day when, if sewage irrigation is not soon established for all large towns, all our rivers will become no better than foul open sewers. It will also be only a question of time, of somewhat greater length, when the amount of sewage will counterbalance the effect of the greater volume of water in the Delaware river also.

Moreover, we have no reason to suppose at the present time that the specific cause of infectious disease may not co-exist with a small amount of organic pollution as well as with a large amount of it. We do not know whether this specific cause may not be capable of resisting all oxidizing influence for long periods of time, although the probabilities in some cases seem to be against this.

There is a marked tendency, of latter times, to give up rivers as a source of public water supply and to seek for it in natural and artificial lakes or large reservoirs in hilly districts. By this plan use is made of the storm waters of the region, and a water is obtained as nearly pure from the clouds by natural distillation as can be done on

a large scale. The hill country from which the supply is taken must be chiefly in woodland or pasture ground, and should not be in active cultivation. It is also to be noted that sewage contamination is less likely to occur in such a lake than in a river, for it has been observed in many places and among them, Massachusetts, that the centres of population extend usually along the river courses and seldom settle on the shores of small lakes. Hence, the plan of building an immense impounding reservoir on the drainage area, or, what is improperly termed the "water-shed" of Perkiomen creek, appears in some respects to be the best for the permanent future supply of Philadelphia, not only as regards its purity but also in regard to quantity of water. But the whole subject of this plan requires a far more thorough and extensive investigation than has yet been made. The teachings of sanitary science in all its bearings must be applied and investigations must be made as to what pollutions appear to exist at the present time in the Perkiomen creek and what is probable in the distant future. The question of the necessity of a State Board of Health must be considered, and the requisite control such a board should have in maintaining the purity of the water supply. For should polluting material enter this artificial lake, that which will not subside will be less likely to be thoroughly oxidized than in the river, and the pollution is, therefore, more important in the former case. Moreover, the continual subsidence of organic impurity in a reservoir which cannot be cleaned is an element of danger.

Comparative Expense of Lighthouse Service.—Emile Allard has published a comparison of the principal expenditures for lighthouse service in France, the United States and England. He finds the average annual cost of each light to be 3580 francs ($716) in France and 11,790 francs ($2358) in the United States. A large part of the economy in the French service is undoubtedly due to the difference in the cost of labor; but he also thinks that much of it is owing to the vigorous economy which the engineers of the department of bridges and highways bring to the execution of their labors, and to their careful avoidance of introducing luxurious arrangements, which do not contribute to manifest utility.—*Annales des. Ponts et Chaussées.* C.

A FOURTH STATE OF MATTER.

By ALEXANDER E. OUTERBRIDGE, JR.

A lecture delivered before the Franklin Institute, February 17th, 1881.*

While the past score of years has been fruitful in producing brilliant inventions like the telephone and other practical applications of science to utilitarian purposes, we should likewise recognize that great progress has been made in the less popular, though no less important, realms of purely abstract and theoretical science. It is a significant observation that all modern research tends to simplify nature's laws, to show their intimate correlation, and even to point still farther toward a complete unification or oneness of origin; thus the great forces of light, heat and electricity, though differing never so widely in their effect on our senses, have all been resolved into "modes of motion," and it would almost seem a natural inference from the general drift of modern scientific speculation that the now seemingly complex laws of nature's forces may all come, at some future day, to be included in the single study of the laws of motion. This little word *motion*, then, may be regarded as the key-note, the open sesame, of the modern philosopher. Even the so-called "inert matter" has not escaped its quickening influence. Speculations, at first regarded as visionary, are gradually formulating themselves into an at least plausible theory that "that which we call matter may be nothing more than the effect on our senses of the *movements* of molecules, or, as John Stuart Mill expresses it, a permanent possibility of sensation."

Sir Wm. Thomson, the profound scientist and mathematician, who may be regarded as a moulder of men's minds, has advanced a startling theory, which, however, is gaining constantly new proselytes, viz.:

* The Secretary of the Institute said, in introducing the lecturer, that a little more than one year ago Prof. Crookes, of the Royal Society, delivered a very remarkable lecture before the "British Association for the Advancement of Science," under the striking title "Radiant Matter." This lecture has excited universal scientific attention not only from the fact that whole classes of entirely new phenomena were developed, but also from the exceeding beauty and originality of the experimental illustrations. A few months ago a full set of Prof. Crookes' delicate apparatus was imported by Messrs. Queen & Co., of this city, and exhibited at one of the monthly meetings of the Franklin Institute, and a resolution was then passed inviting Mr. Outerbridge to lecture on the subject.

"That what we call matter may be only the rotating portions of something which fills the whole of space, *i. e.*, 'vortex motion of an everywhere present fluid.' "

Prof. Tait, following in the same path, says: "This property of rotation may be the basis of all that appeals to our senses as matter."

Prof. Crookes says: "From this point of view, then, matter is but a mode of motion." *

We are quite certain that many of the physical, as well even as some chemical changes which we observe in the sensible condition of matter, are due merely to movements among the molecules, or to motion of the atoms; thus the three commonly recognized states of matter, viz.: solid, liquid and gaseous, are consequences simply of the greater or lesser amplitude of motion of the constituent particles, and the question whether matter may exist in a fourth, or *ultra-gaseous* state, is the great scientific conundrum which Prof. Crookes has essayed to answer in the affirmative. He believes that he has discovered matter in a state as far removed from the gaseous as gas is from liquid or liquid from solid, and says: "In studying this fourth state of matter we seem at length to have within our grasp, and obedient to our control, the little indivisible particles of matter which, with good warrant, are supposed to constitute the physical basis of the universe." Whether Prof. Crookes' conclusions prove conclusive or not, we are indebted to his genius for a most brilliant investigation, teeming with possibilities

* "Motion, wherever we can directly trace its genesis, we find to pre-exist as some other mode of force. Our own voluntary acts have always certain sensations of muscular tension as their antecedents. When, as in letting fall a relaxed limb, we are conscious of a bodily movement requiring no effort, the explanation is that the effort was exerted in raising the limb to the position whence it fell. In this case, as in the case of an inanimate body descending to the earth, the force accumulated by the downward motion is just equal to the force previously expended in the act of elevation.

"Conversely, motion that is arrested produces, under different circumstances, heat, electricity, magnetism, light. From the warming of the hands by rubbing them together up to the ignition of a railway brake by intense friction—from the lighting of detonating powder by percussion, up to the setting on fire a block of wood by a few blows from a steam hammer; we have abundant instances in which heat arises as motion ceases. * * * The production of electricity by motion is illustrated equally in the boy's experiment with rubbed sealing wax, in the common electrical machine and in the apparatus for exciting electricity by the escape of steam. * * * * And similarly, motion may create light, either directly, as in the minute incandescent fragments struck off by violent collisions, or indirectly, as through the electric spark. 'Lastly, motion may be again reproduced by the forces which have emanated from motion.' "—*Synthetic Philosophy*, Herbert Spencer, p. 197.

for further development and giving us fascinating little vistas into unexplored regions of knowledge in that "border land where Matter and Force seem to merge into one another, the shadowy realm between Known and Unknown."

Inter-Molecular Spaces and Motion.

The porosity of all substances, even those which we commonly regard as most dense, proves that the constituent particles are not in contact. You remember the old experiment of the sphere of gold filled with water, on compressing the sphere drops of water oozed through the pores of the gold, *i. e.*, the spaces between the molecules.

A bar of iron, or other metal, may be readily compressed under a steam hammer, and we know that we do not diminish the size of the metallic particles one iota; we simply contract their house room, as it were, or crowd them nearer together.

The porosity of liquids may be readily proved,* while this property is one of the most striking characteristics of all gases.†

The idea that the constituent molecules of all bodies, whether in the solid liquid or gaseous state, are not at rest, but constantly moving, with enormous velocities, within certain boundaries, is so inseparably associated with our modern conception of matter in these three states that we may say, broadly, we believe firmly that no solitary molecule, whether constituting an infinitesimal portion of this great globe or inhabiting the most distant star, has yet found a lodging place where it may enjoy absolute rest. The particle was endowed at its creation with an irresistible antagonism to inertness. Constant motion is a very necessity of its being; the whirling and the clashing of the atoms goes on forever, like the music of the spheres, and this eternal dance of molecules can cease only with the utter annihilation of familiar matter.‡

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* *E g.* A glass filled with water, to which may be added salt, sugar, etc.

† *E g.* Diffusion exhibited by Prof. Graham's experiment, etc.

‡ "The space covered by the motion of molecules has no more right to be called matter than the air, traversed by a rifle bullet, can be called lead.. * * * At the absolute zero of temperature the inter-molecular movement would stop, and, although *something* retaining the properties of inertia would remain, *matter*, as we know it, would cease to exist."—Letter to secretary of the Royal Society, W. Crookes, F.R.S., *Nature*, vol. 22, No. 7, p. 153.

"There is no repose of any kind in nature; its whole existence is a constant cycle, in which every motion, the consequence of a preceding motion, becomes immediately

The change of matter from the solid to the liquid and then to the gaseous state is a most familiar occurrence. Let us try to form a clear mental picture of the movements, which the molecules undergo, whereby a body which is opaque like lead and rigid like iron becomes first plastic, then fluid, and then dissipates before our eyes as invisible vapor. Whence come these marvelous transmutations? A crude illustration may perhaps answer our question clearly and without scientific technicality. Let us imagine a hall crowded to repletion with dancers. It is evident that the movements of each person will be very circumscribed, he cannot advance far in any direction without jostling his neighbor; the result will be that, at the end of a given time, the individuals will all occupy the same relative positions in the room that they held at the beginning. The characteristics of the mass, therefore, have remained unchanged. This simile illustrates the motion of the molecules composing a solid body; they may be regarded as gyrating within small limits of space and constantly jostling each other; their paths of free movement being exceedingly small, the molecules cannot change places, and the characteristics of hardness, rigidity, etc., remain fixed.

Now, let us open the folding doors of the hall so that some of the dancers may overflow into the adjoining room; more freedom of movement is immediately afforded, the individuals change places constantly in this giddy whirl, and an observer in the gallery notices kaleidoscopic changes every moment in the characteristics of the mass. This represents the change of a solid body to the condition of a fluid.

If we continue to enlarge the boundaries, still greater changes will be observed in the mass; the unoccupied space becomes excessive, so that

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the cause of an equivalent succeeding one; so that there is nowhere a gap, nowhere either loss or gain."—Buchner.

"Matter is not like a carriage, to which the forces, like horses, can be put or again removed from. A particle of iron is, and remains, the same whether it crosses the horizon in the meteoric stone, rushes along in the wheel of the steam engine, or circulates in the blood through the temples of the poet. These qualities are eternal, inalienable and untransferable."—Du Bois-Reymond.

"In whatever way we may think of an original substance, there must always exist in it a system of mutual repulsion and attraction between its minutest parts, without which they would dissolve and tracelessly disappear in universal space."—Buchner.

"A thing without properties is a nonentity, neither rationally cogitable nor empirically exsisting in nature.—Drossbach.

"A force not united to matter, but floating freely above it, is an idle conception.—Moleschott.

"the mean free path" of each individual is greatly increased, and collisions can only occur because the waltz merges into a wild galop. This depicts the molecules of matter in the gaseous or third state.

If we should be able still further to isolate the individual dancers, or what is equivalent, to greatly decrease their number, each one would have so vast an allotment of space, or the "mean free path" of each would become so great that we could no longer observe the motions of a group *en masse*, but should have to confine our attention to one, or at least a very small number who could only communicate with or jostle against each other by an exercise of agility hitherto quite unsuspected, fairly entitling them to be called by a new name, as acrobats rather than dancers. Such may serve as a homely illustration of the modern idea of matter as it is said to exist in a "fourth" or "radiant" or "ultra-gaseous" state.

Divisibility of Matter.

In studying the character of the ultimate particles we are at once impressed with the extraordinary degree to which matter may be subdivided. Numerous illustrations might be given had we sufficient time and space to devote to this interesting by-path. "Some of these experiments (notably those of Faraday) present the curious anomaly of revealing to the physical sense of sight particles of matter, which are almost too infinitesimal for the mind's eye to conceive, thus seeming to reverse the order of scientific investigation, which usually prolongs the mental vision far beyond the region of possible physical revelation."*

By mere mechanical means the metal gold may be spread out into a leaf one three-hundred-and-fifty-thousandths of an inch in thickness.

By means of the battery films of gold have been deposited on copper and afterward detached, by dissolving the copper and floating the gold on glass slides, not exceeding one ten-millionth of an inch in thickness.

By the aid of the spectroscope particles of matter may be revealed which are invisible to the eye by the aid of the most powerful microscope.

We are assured that there are infusorial animals so minute that millions do not equal in bulk a single grain of sand.

Tripoli consists of the bodies of animalcules so small that a piece

* Divisibility of gold and other metals, by A. E. O., Jr., *Pop. Science Monthly*, May, 1877.

the size of a marble contains at least 1,000,000,000, or more than all the human beings on the globe. "Each animalcule possessed all the organs necessary to life, performing the functions of respiration, digestion and locomotion; how inconceivably small are the vessels through which the fluids of their bodies circulate, and what must be the size of the ultimate particles to nourish an animal whose totality is too small to estimate?"

Compared, however, to the size of the particles constituting cometary matter, those which we have here indicated are as coarse as the famous Philadelphia cobble stones contrasted with grains of sand upon the seashore.*

We might naturally suppose from these considerations that no absolute knowledge could ever be obtained regarding the nature of the infinitesimal particles of matter out of which worlds are formed; indeed, Brande has said "Of the ultimate nature of matter the human faculties can not take cognizance, nor can data be furnished by observation or experiment on which to found an investigation of it." Sir Wm. Thomson, however, has said more recently that they are "pieces of matter of measurable dimensions, with shape, motion and laws of action; intelligible subjects of scientific investigation."

The same eminent authority, undaunted by the intricate complexities of the problem, has after most careful research, and by the corro-

* "From their perviousness to stellar light and other considerations Sir Jno. Herschell drew some startling conclusions regarding the density and weight of comets. These extraordinary and mysterious bodies sometimes throw out tails 100,000,000 miles in length and 50,000 miles in diameter. Now, suppose the whole of this stuff (*i. e.*, the matter forming the tail) to be swept together and suitably compressed, what do you suppose its volume would be? Sir Jno. Herschell would probably tell you that the whole mass might be carted away, at a single effort, by one of your dray-horses. In fact, I do not know that he would require more than a small fraction of a horse power to remove the cometary dust.

"After this you will hardly regard as montrous a notion I have sometimes entertained, concerning the quantity of matter in our sky. Suppose a shell to surround the earth at a distance which would place it beyond the grosser matter that hangs in the lower regions of the air, say at the height of the Matterhorn or Mont Blanc. Outside this shell we have the deep, blue firmament. Let the atmospheric space beyond the shell be swept clean and the sky-matter properly gathered up. What would be its probable amount? I have sometimes thought that a lady's portmanteau would contain it all. I have thought that even a gentleman's portmanteau, possibly his snuff box, might take it in. Whether the actual sky be capable of this amount of condensation or not, I entertain no doubt that a sky quite as vast as ours, and as good in appearance, can be formed from a quantity of matter which might be held in the hollow of the hand."—*Fragments of Science*, Tyndall, p. 444.

borative aid of at least four separate and distinct methods of calculation, all agreeing within certain limits of error, deduced approximately the size of the ultimate particle, which he tells us is not far from one five-hundred-millionth of an inch in diameter. To form an idea of the size of these molecules Sir Wm. Thomson has given this illustration: "Imagine a drop of rain, or a glass sphere the size of a pea, magnified to the size of the earth, the molecules in it being increased in the same proportion, the structure of the mass would then be coarser than that of a heap of fine shot, but probably not so coarse as that of a heap of cricket balls.

This estimate is commonly received by scientists as representing the average "coarse-grainedness" of matter.

Nature of Matter.

We are now prepared to ask what is the character of the ultimate particle? This is a problem which has exercised the philosophic mind for many generations. The celebrated Italian poet and philosopher, Lucretius, speculated on this topic, and left a work called "De Rerum Natura"—*of the nature of things*—embodying his views, which are interesting even at this day.

Sir Isaac Newton invented, or adopted, the theory that all matter was composed of little, hard, incompressible spheres. This theory afforded him a plausible and ingenious explanation of certain discrepancies which he found to exist between the real velocity of sound in air, as proved by experiment, compared with the theoretical velocity, as calculated by him, based upon seemingly correct data. The theory, however, was found to be untenable, and was long since overthrown.

The idea of motion, as associated intimately with matter in some vague and unexplained way, seemed to be intuitively felt to be a necessity even before the time of Faraday, and a reaction from the hard atom theory of Newton brought out a class of philosophers who maintained that the so-called atom was not material at all, but that there existed in space certain foci, or centres of force; these points were supposed to possess the properties of attraction, repulsion, etc., and to behave in other respects as the most approved atom. This idea is believed to have been at least countenanced, if not actually adopted, by Faraday.

The wonderful investigations of Helmholtz on rotary fluid motion paved the way for Sir Wm. Thomson's "Vortex Atom Theory."

Vortex Atom Theory.

This theory seems to explain many obscure phenomena, and, while we may hardly say that it is to-day the fully accepted creed of the scientific world, it is founded—not upon a rock—but upon the potent influence of the talismanic word motion, to whose eddying current the causes of so many of nature's grandest phenomena are now relegated.

"Sir Wm. Thomson's supposition is that the universe is filled with something which we have no right to call ordinary matter, but which we may call a perfect fluid, then, if any portions of it have vortex motion, they cannot part with it; it will remain with them forever, or at least until the creative act which produced it shall take it away again."*

This theory is far too complicated to admit of a clear exposition in a few words, but we will try to form an idea of it by means of a simple illustration. You know that a smoker may, by a peculiar adjustment of the mouth, accompanied by sudden muscular movements of the cheeks, skilfully exhale portions of the tobacco smoke in the form of beautiful opaque rings, which continue to rotate about their axes, gradually disappearing as the motion ceases; this is a vortex movement of the simplest order, and while it lasts the portions of the smoke forming the rings are separated and distinct from all the rest of the smoke in the room, and should this motion never cease (by the retarding influence of friction and gravitation) this smoke would remain differentiated forever and imbued with new properties in virtue of that vortex motion.

This crude illustration conveys the simplest idea of a most abstruse scientific speculation when referred to the hypothetical, all-pervading, perfect fluid, so essential to the existence of Sir Wm. Thomson's vortex atom.†

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* *Recent Advances in Science*, P. G. Tait.

† The subject of rotary fluid motion is such a forbidding one, from a purely mathematical point of view, that no one had done more than take a look at it, as it were, until Helmholtz gave us the fundamental propositions; splendid as they are, they are only a first step. Indeed, to investigate what takes place when one circular vortex atom impinges upon another, and the whole motion is not symmetrical about an axis, is a task which may employ perhaps the lifetimes for the next two or three generations of the best mathematicians in Europe, unless in the meantime some mathematical method, immensely more powerful than anything we at present have, should be devised for the special purpose of solving this problem.—P. G. Tait, *Recent Advances*, p. 298.

The numerous criticisms which have appeared of Prof. Crookes' remarkable lecture on radiant matter have all, strange to say, failed to recognize the fact, so clearly and completely stated by Prof. Crookes himself, that the original idea of an ultra-gaseous condition of matter is by no means a novelty, having been conceived by Faraday, who mentioned this extremely attenuated form as radiant matter as far back as 1816.*

The concluding portion of the lecture was devoted to an exhibition and explanation of Prof. Crookes' apparatus, contributed for this occasion by Messrs. Queen & Co. This consists, for the most part, of a number of small glass tubes and bulbs, from which the air has been exhausted to about one-millionth part of an atmosphere; fine wires are hermetically sealed into the tubes and a powerful induction coil supplies rapid electrical pulsations, under the influence of which the residual matter, or particles of air, and even the sides of the glass flash out into brilliant corruscations of phosphorescent light, of exquisite tints, exhibiting also mechanical and other effects of a surprising and entirely novel character.

Before showing the apparatus of Prof. Crookes, some very beautiful experiments with low vacuum tubes were exhibited by way of contrast with those which followed. The Crookes tubes included one showing the dark space where a plate of aluminium in the centre of the tube was the negative pole, and the radiant matter driven forth by the current only exhibited itself by light when in the part of the tube where the moving atoms were brought into collision with the sides of the tube or with each other. Two or three minute engines, showing the

* "It was more than sixty years afterwards that Prof. Crookes took up the subject in a way to make the phrase "radiant matter" familiar to the reading public, as well as to the guild of scientific men. This "fourth state," or extra-gaseous state, of matter was not produced in his experiments by the application of high heat, as in melting metals or turning water into vapor, but by pumping the air from glass tubes and vessels until only the one-millionth of an ordinary atmosphere was left within. In this condition what remained of the air in the tubes and vessels was so greatly diffused that each individual molecule of air had a million times more freedom of movement than in the natural condition of the atmosphere, and it is the very remarkable character of these enclosed molecules in a high vacuum when excited by external agencies, such as electricity, that has caused Prof. Crookes to designate them as "radiant matter," following Faraday, and "a fourth state of matter," following the phraseology of other scientists. The widely separated atoms are made to act with marvelous energy and to produce wonderful effects—they not only produce light and heat by their motion, but while invisible themselves, they are made to set visible things in motion and to produce other visible effects.—Editorial in *Public Ledger Supplement*, Feb. 19, 1881.

mechanical force of radiant matter, were also exhibited. One of them was a propeller wheel, which, at first, refused to revolve, though evidently agitated by the current. It was afterwards ascertained that the wire connection had been broken, and the accident afforded a good illustration of the theory of the action produced, the spasmodic movements of the wheel being due to the jumping of the current between the ends of the broken wire. The connections having been properly made, the wheel revolved. Another tube, which was projected from a lantern, has placed within it a miniature railway track of glass, on which there is a small wheel, like the side wheel of a steamboat, the blades or paddles being made of thin mica, and when the terminal wires at the ends of the tube are connected with an induction coil the infinitesimal molecules are shot from one end of the tube to the other, striking the blades on their way with such force, and exhibiting such rapid motion, as to carry the wheel from end to end and back in swift alternation as the current of electricity is reversed in polarity. The shadow tube was also shown. In this a Maltese cross is arranged in the tube, which intercepts the moving atoms and so produces a shadow on the end of the tube, which is illuminated outside of the lines of the cross by the impinging of the particles on the end of the tube. The cross being thrown down, the former shadow becomes brighter than the surrounding parts. It is supposed that the glass, having been subjected to the action, becomes less sensitive, so that the newly exposed parts for a time respond with greater vibrations and so give more light. Tubes showing that in the lower vacuum the particles travelled in direct lines between the terminals while in higher vacuums they were driven in straight lines directly from the negative pole, were also shown. Another tube was made with two chambers, in one of which were the electrodes and in the other a compound of potassium, capable of giving out a watery vapor. The tube was exhausted to the last degree, so that the passage of the current was not possible. Then the substance in the "annex" chamber was heated to make it give off atoms or vapor when the particles of the glass were set in violent motion and gave forth the characteristic green light. As the number of molecules and consequent collisions increased this color changed to the rosy hue seen in low vacuum tubes. As the vapors were recondensed the green light returned and finally the current ceased to pass and all was darkness. The last experiment was with a tube in which the radiant matter, passing from a cup-shaped negative terminal, was focussed, so

to speak, on a metal disc of iridio-platinum, heating it to redness. It was shown with this tube that the direction of the atoms could be deflected by a magnet. The experiments were as beautiful as they were instructive.

ACTION OF AN INTERMITTENT BEAM OF RADIANT HEAT UPON GASEOUS MATTER.

By JOHN TYNDALL, F.R.S.

Read before the Royal Society January 3, 1881.

The Royal Society has already done me the honor of publishing a long series of memoirs on the interaction of radiant heat and gaseous matter. These memoirs did not escape criticism. Distinguished men, among whom the late Professor Magnus and the late Professor Buff may be more specially mentioned, examined my experiments, and arrived at results different from mine. Living workers of merit have also taken up the question, the latest of whom,* while justly recognizing the extreme difficulty of the subject, and while verifying, so far as their experiments reached, what I had published regarding dry gases, find me to have fallen into what they consider grave errors in my treatment of vapors.

None of these investigators appear to me to have realized the true strength of my position in its relation to the objects I had in view. Occupied for the most part with details, they have failed to recognize the stringency of my work as a whole, and have not taken into account the independent support rendered by the various parts of the investigation to each other. They thus ignore verifications, both general and special, which are to me of conclusive force. Nevertheless, thinking it due to them and me to submit the questions at issue to a fresh examination, I resumed, some time ago, the threads of the inquiry. The results shall, in due time, be communicated to the Royal Society; but, meanwhile, I would ask permission to bring to the notice of the Fellows a novel mode of testing the relations of radiant heat to gaseous matter, whereby singularly instructive effects have been obtained.

After working for some time with the thermopile and galvanometer, it occurred to me several weeks ago that the results thus obtained

*MM. Lecher and Pernter, *Philosophical Magazine*, January, 1881; *Sitzb. der K. Akad. der Wissensch. in Wien*, July, 1880.

might be checked by a more direct and simple form of experiment. Placing the gases and vapors in diathermanous bulbs, and exposing the bulbs to the action of radiant heat, the heat absorbed by different gases and vapors ought, I considered, to be rendered evident by ordinary expansion. I devised an apparatus with a view of testing this idea; but, at this point, and before my proposed gas-thermometer was constructed, I became acquainted with the ingenious and original experiments of Mr. A. Graham Bell, wherein musical sounds are obtained through the action of an intermittent beam of light upon solid bodies.

From the first, I entertained the opinion that these singular sounds were caused by rapid changes of temperature, producing corresponding changes of shape and volume in the bodies impinged upon by the beam. But if this be the case, and if gases and vapors really absorb radiant heat, they ought to produce sounds more intense than those obtainable from solids. I pictured every stroke of the beam responded to by a sudden expansion of the absorbent gas, and concluded that when the pulses thus excited followed each other with sufficient rapidity, a musical note must be the result. It seemed plain, moreover, that by this new method many of my previous results might be brought to an independent test. Highly diathermanous bodies, I reasoned, would produce faint sounds, while highly athermanous bodies would produce loud sounds; the strength of the sound being, in a sense, a measure of the absorption. The first experiment made, with a view of testing this idea, was executed in the presence of Mr. A. Graham Bell;* and the result was in exact accordance with what I had foreseen.

The inquiry has been recently extended so as to embrace most of the gases and vapors employed in my former researches. My first source of rays was a Siemens' lamp connected with a dynamo machine, worked by a gas engine. A glass lens was used to concentrate the rays, and afterwards two lenses. By the first the rays were rendered parallel, while the second caused them to converge to a point about 7 inches distant from the lens. A circle of sheet zinc, provided first with radial slits and afterwards with teeth and interspaces cut through it, was mounted vertically on a whirling table, and caused to rotate rapidly across the beam near the focus. The passage of the slits pro-

* On the 29th of November; see *Journal of the Society of Telegraphic Engineers*, Dec. 8th, 1880.

duced the desired intermittence,* while a flask containing the gas or vapor to be examined received the shocks of the beam immediately behind the rotating disc. From the flask a tube of india rubber, ending in a tapering one of ivory or boxwood, led to the ear, which was thus rendered keenly sensitive to any sound generated within the flask. Compared with the beautiful apparatus of Mr. A. Graham Bell, the arrangement here described is rude; it is, however, very effective.

With this arrangement the number of sounding gases and vapors was rapidly increased; but I was soon made aware that the glass lenses withdrew from the beam its most effectual rays. The silvered mirrors employed in my previous researches were therefore invoked, and with them, acting sometimes singly and sometimes as conjugate mirrors, the curious and striking results, which I have now the honor to submit to the Society, were obtained.

Sulphuric ether, formic ether and acetic ether, being placed in bulbous flasks,† their vapors were soon diffused in the air above the liquid. On placing these flasks, whose bottoms only were covered by the liquid, behind the rotating disc, so that the intermittent beam passed through the vapor, loud musical tones were in each case obtained. These are known to be the most highly absorbent vapors which my experiments revealed. Chloroform and bisulphide of carbon, on the other hand, are known to be the least absorbent, the latter standing near the head of diathermanous vapors. The sounds extracted from these two substances were usually weak and sometimes barely audible, being more feeble with the bisulphide than with the chloroform. With regard to the vapors of amylene, iodide of ethyl, iodide of methyl and benzol, other things being equal, their power to produce musical tones appeared to be accurately expressed by their ability to absorb radiant heat.

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*When the disc rotates the individual slits disappear, forming a hazy zone, through which objects are visible. Throwing by the clean hand, or, better still, by white paper, the beam back upon the disc, it appears to stand still, the slits forming so many dark rectangles. The reason is obvious, but the experiment is a very beautiful one.

I may add, that when I stand with open eyes in the flashing beam, at a definite velocity of recurrence, subjective colors of extraordinary gorgeousness are produced. With slower or quicker rates of rotation the colors disappear. The flashes also produced a giddiness sometimes intense enough to cause me to grasp the table to keep myself erect.

†I have employed flasks measuring from 8 inches to three-fourths of an inch in diameter. The smallest flask, which had a stem with a bore of about one-eighth of an inch in diameter, yielded better effects than the largest. Flasks from 2 to 3 inches in diameter yield good results. Ordinary test-tubes also answer well.

It is the vapor, and not the liquid, that is effective in producing the sounds. Taking, for example, the bottles in which my volatile substances are habitually kept, I permitted the intermittent beam to impinge upon the liquid in each of them. No sound was in any case produced, while the moment the vapor-laden space above an active liquid was traversed by the beam, musical tones made themselves audible.

A rock salt cell filled entirely with a volatile liquid, and subjected to the intermittent beam, produced no sound. This cell was circular, and closed at the top. Once, while operating with a highly athermanous substance, a distinct musical note was heard. On examining the cell, however, a small bubble was found at its top. The bubble was less than a quarter of an inch in diameter, but still sufficient to produce audible sounds. When the cell was completely filled the sounds disappeared.

It is hardly necessary to state that the pitch of the note obtained in each case is determined by the velocity of rotation. It is the same as that produced by blowing against the rotating disc, and allowing its slits to act like the perforations of a syren.

Thus, as regards vapors, prevision has been justified by experiment. I now turn to gases. A small flask, after having been heated in the spirit lamp so as to detach all moisture from its sides, was carefully filled with dried air. Placed in the intermittent beam, it yielded a musical note, but so feeble as to be heard only with attention. Dry oxygen and hydrogen behaved like dry air. This agrees with my former experiments, which assigned a hardly sensible absorption to these gases. When the dry air was displaced by carbonic acid, the sound was far louder than that obtained from any of the elementary gases. When the carbonic acid was displaced by nitrous oxide the sound was much more forcible still, and when the nitrous oxide was displaced by olefiant gas it gave birth to a musical note which, when the beam was in good condition and the bulb well-chosen, seemed as loud as that of an ordinary organ pipe.* We have here the exact order in which my former experiments proved these gases to stand as absorbers of radiant heat. The amount of the absorption and the intensity of the sound go hand-in-hand.

*With conjugate mirrors the sounds with olefiant gas are readily obtained at a distance of twenty yards from the lamp. I hope to be able to make a candle flame effective in these experiments.

A soap bubble blown with nitrous oxide or olefiant gas, and exposed to the intermittent beam, produced no sound, no matter how its size might be varied. The pulses obviously expended themselves upon the flexible envelope, which transferred them to the air outside.

But a film thus impressionable to impulses on its interior surface must prove at least equally sensible to sonorous waves impinging on it from without. Hence, I inferred, the eminent suitability of soap bubbles for sound lenses. Placing a "sensitive flame" some feet distant from a small sounding reed, the pressure was so arranged that the flame burnt tranquilly. A bubble of nitrous oxide (sp. gr. 1·527) was then blown and placed in front of the reed. The flame immediately fell and roared, and continued agitated as long as the lens remained in position. A pendulous motion could be imparted to the bubble, so as to cause it to pass to and fro in front of the reed. The flame responded by alternately roaring and becoming tranquil to every swing of the bubble. Nitrous oxide is far better for this experiment than carbonic acid, which speedily ruins its envelope.

The pressure was altered so as to throw the flame, when the reed sounded, into violent agitation. A bubble blown with hydrogen (sp. gr. 0·069) being placed in front of the reed, the flame was immediately stilled. The ear answers instead of the flame.

In 1859 I proved gaseous ammonia to be extremely impervious to radiant heat. My interest in its deportment when subjected to this novel test was therefore great. Placing a small quantity of liquid ammonia in one of the flasks, and warming the liquid slightly, the intermittent beam was sent through the space above the liquid. A loud musical note was immediately produced. By the proper application of heat to a liquid the sound may be always intensified. The ordinary temperature, however, suffices in all the cases thus far referred to.

In this relation the vapor of water was that which interested me most, and as I could not hope that at ordinary temperatures it existed in sufficiant amount to produce audible tones, I heated a small quantity of water in a flask almost up to its boiling point. Placed in the intermittent beam, I heard—I avow with delight—a powerful musical sound produced by the aqueous vapor.

Small wreaths of haze, produced by the partial condensation of the vapor in the upper and cooler air of the flask, were, however, visible in this experiment; and it was necessary to prove that this haze was not

the cause of the sound. The flask was, therefore, heated by a spirit-flame beyond the temperature of boiling water. The closest scrutiny by a condensed beam of light then revealed no trace of cloudiness above the liquid. From the perfectly invisible vapor, however, the musical sound issued, if anything, more forcible than before. I placed the flask in cold water until its temperature was reduced from about 90° to 10°C., fully expecting that the sound would vanish at this temperature; but, notwithstanding the tenuity of the vapor, the sound extracted from it was not only distinct, but loud.

Three empty flasks, filled with ordinary air, were placed in a freezing mixture for a quarter of an hour. On being rapidly transferred to the intermittent beam, sounds much louder than those obtainable from dry air were produced.

Warming these flasks in the flame of a spirit-lamp until all visible humidity had been removed, and afterwards urging dried air through them, on being placed in the intermittent beam the sound in each case was found to have fallen almost to silence.

Sending, by means of a glass tube, a puff of breath from the lungs into a dried flask, the power of emitting sound was immediately restored.

When, instead of breathing into a dry flask, the common air of the laboratory was urged through it, the sounds became immediately intensified. I was by no means prepared for the extraordinary delicacy of this new method of testing the athermancy and diathermancy of gases and vapors, and it cannot be otherwise than satisfactory to me to find that particular vapor, whose alleged deportment towards radiant heat has been so strenuously denied, affirming thus audibly its true character.

After what has been stated regarding aqueous vapor, we are prepared for the fact that an exceedingly small percentage of any highly athermanous gas diffused in air suffices to exalt the sounds. An accidental observation will illustrate this point. A flask was filled with coal-gas, and held bottom upwards in the intermittent beam. The sounds produced were of a force corresponding to the known absorptive energy of coal-gas. The flask was then placed upright, with its mouth open, upon a table, and permitted to remain their for nearly an hour. On being restored to the beam, the sounds produced were far louder than those which could be obtained from common air.*

* The method here described is, I doubt not, applicable to the detection of extremely small quantities of fire-damp in mines.

Transferring a small flask or a test-tube from a cold place to the intermittent beam, it is sometimes found to be practically silent for a moment, after which the sounds become distinctly audible. This I take to be due to the vaporization by the calorific beam of the thin film of moisture adherent to the glass.

My previous experiments having satisfied me of the generality of the rule that volatile liquids and their vapors absorb the same rays, I thought it probable that the introduction of a thin layer of its liquid, even in the case of a most energetic vapor, would detach the effective rays, and thus quench the sounds. The experiment was made, and the conclusion verified. A layer of water, formic ether, sulphuric ether or acetic ether, one-eighth of an inch in thickness, rendered the transmitted beam powerless to produce any musical sound. These liquids being transparent to light, the efficient rays which they intercepted must have been those of obscure heat.

A layer of bisulphide of carbon, about ten times the thickness of the transparent layers just referred to, and rendered opaque to light by dissolved iodine, was interposed in the path of the intermittent beam. It produced hardly any diminution of the sounds of the more active vapors—a further proof that it is the invisible heat rays, to which the solution of iodine is so eminently transparent, that are here effectual.

Converting one of the small flasks used in the foregoing experiments into a thermometer bulb, and filling it with various gases in succession, it was found that with those gases which yielded a feeble sound the displacement of a thermometric column associated with the bulb was slow and feeble, while with those gases which yielded loud sounds the displacement was prompt and forcible.

Further Experiments.—Since the handing in of the foregoing note, on the 3d of January, the experiments have been pushed forward; augmented acquaintance with the subject serving only to confirm my estimate of its interest and importance.

All the results described in my first note have been obtained in a very energetic form with a battery of 60 Grove's cells.

On the 4th of January I chose for my source of rays a powerful lime-light, which, when sufficient care is taken to prevent the pitting of the cylinder, works with admirable steadiness and without any noise. I also changed my mirror for one of shorter focus, which permitted a nearer approach to the source of rays. Tested with this new reflector the stronger vapors rose remarkably in sounding power.

Improved manipulation was, I considered, sure to extract sounds from rays of much more moderate intensity than those of the lime-light. For this light, therefore, a common candle flame was substituted. Received and thrown back by the mirror, the radiant heat of the candle produced audible tones in all the stronger vapors.

Abandoning the mirror and bringing the candle close to the rotating disc, its direct rays produced audible sounds.

A red-hot coal, taken from the fire and held close to the rotating disc, produced forcible sounds in a flask at the other side.

A red-hot poker, placed in the position previously occupied by the coal, produced strong sounds. Maintaining the flask in position behind the rotating disc, amusing alternations of sound and silence accompanied the alternate introduction and removal of the poker.

The temperature of the iron was then lowered till its heat just ceased to be visible. The intermittent invisible rays produced audible sounds.

The temperature was gradually lowered, being accompanied by a gradual and continuous diminution of the sound. When it ceased to be audible the temperature of the poker was found to be below that of boiling water.

As might be expected from the foregoing experiments, an incandescent platinum spiral, with or without the mirror, produced musical sounds. When the battery power was reduced from ten cells to three the sounds, though enfeebled, were still distinct.

My neglect of aqueous vapor has led me for a time astray in 1859, but before publishing my results I had discovered my error. On the present occasion this omnipresent substance had also to be reckoned with. Fourteen flasks of various sizes, with their bottoms covered with a little sulphuric acid, were closed with ordinary corks, and permitted to remain in the laboratory from the 23d of December to the 4th of January. Tested on the latter day with the intermittent beam, half of them emitted feeble sounds, but half were silent. The sounds were undoubtedly due not to dry air, but to traces of aqueous vapor.

An ordinary bottle, containing sulphuric acid for laboratory purposes, being connected with the ear and placed in the intermittent beam, emitted a faint but distinct musical sound. This bottle had been opened two or three times during the day, its dryness being thus vitiated by the mixture of a small quantity of common air. A second similar bottle, in which sulphuric acid had stood undisturbed for some

days, was placed in the beam; the dry air above the liquid proved absolutely silent.

On the evening of January the 7th, Professor Dewar handed me four flasks treated in the following manner. Into one was poured a small quantity of strong sulphuric acid; into another a small quantity of Nordhausen sulphuric acid; in a third were placed some fragments of fused chloride of calcium, while the fourth contained a small quantity of phosphoric anhydride. They were closed with well-fitting india rubber stoppers, and permitted to remain undisturbed throughout the night. Tested after twelve hours, each of them emitted a feeble sound, the flask last mentioned being the strongest. Tested again six hours later, the sound had disappeared from three of the flasks, that containing the phosphoric anhydride alone remaining musical.

Breathing into a flask partially filled with sulphuric acid, instantly restores the sounding power, which continues for a considerable time. The wetting of the interior surface of the flask with the sulphuric acid always enfeebles, and sometimes destroys the sound.

A bulb, less than a cubic inch in volume, and containing a little water, lowered to the temperature of melting ice, produces very distinct sounds. Warming the water in the flame of a spirit-lamp the sound becomes greatly augmented in strength. At the boiling temperature the sound emitted by this small bulb* is of extraordinary intensity.

These results are in accord with those obtained by me nearly nineteen years ago, both in reference to air and to aqueous vapor. They are in utter disaccord with those obtained by other experimenters, who have ascribed a high absorption to air and none to aqueous vapor.

The action of aqueous vapor being thus revealed, the necessity of thoroughly drying the flasks, when testing other substances, becomes obvious. The following plan has been found effective. Each flask is first heated in the flame of a spirit-lamp till every visible trace of internal moisture has disappeared, and it is afterwards raised to a temperature of about 400°C. While the flask is still hot a glass tube is introduced into it, and air freed from carbonic acid by caustic potash, and from aqueous vapor by sulphuric acid, is urged through the flask until it is cool. Connected with the ear-tube, and exposed immediately

* In such bulbs even bisulphide of carbon vapor may be so nursed as to produce sounds of considerable strength.

to the intermittent beam, the attention of the ear, if I may use the term, is converged upon the flask. When the experiment is carefully made, dry air proves as incompetent to produce sound as to absorb radiant heat.

In 1868, I determined the absorptions of a great number of liquids whose vapors I did not examine. My experiments having amply proved the parallelism of liquid and vaporous absorption, I held undoubtingly, twelve years ago, that the vapor of cyanide of ethyl and of acetic acid would prove powerfully absorbent. This conclusion is now easily tested. A small quantity of either of these substances, placed in a bulb a cubic inch in volume, warmed and exposed to the intermittent beam, emits a sound of extraordinary power.

I also tried to extract sounds from perfumes, which I had proved, in 1861, to be absorbers of radiant heat. I limit myself here to the vapors of patchouli and cassia, the former exercising a measured absorption of 30 and the latter an absorption of 109. Placed in dried flasks, and slightly warmed, sounds were obtained from both these substances, but the sound of cassia was much louder than that of patchouli.

Many years ago, I had proved tetrachloride of carbon to be highly diathermanous. Its sounding power is as feeble as its absorbent power.

In relation to colliery explosions, the deportment of marsh-gas was of special interest. Professor Dewar was good enough to furnish me with a pure sample of this gas. The sounds produced by it, when exposed to the intermittent beam, were very powerful.

Chloride of methyl, a liquid which boils at the ordinary temperature of the air, was poured into a small flask and permitted to displace the air within it. Exposed to the intermittent beam its sound was similar in power to that of marsh-gas.

The specific gravity of marsh-gas being about half that of air, it might be expected that the flask containing it, when left open and erect, would soon get rid of its contents. This, however, is not the case. After a considerable interval the film of this gas clinging to the interior surface of the flask was able to produce sounds of great power.

A small quantity of liquid bromine being poured into a well-dried flask, the brown vapor rapidly diffused itself in the air above the liquid. Placed in the intermittent beam a somewhat forcible sound was produced. This might seem to militate against my former experiments, which assigned a very low absorptive power to bromine vapor. But my former experiments on this vapor were conducted with obscure

heat; whereas, in the present instance, I had to deal with the radiation from incandescent lime, whose heat is, in part, luminous. Now, the color of the bromine vapor proves it to be an energetic absorber of the luminous rays, and to them, when suddenly converted into thermometric heat in the body of the vapor, I thought the sound might be due.

Between the flask containing the bromine and the rotating disc I therefore placed an empty glass cell; the sounds continued. I then filled the cell with transparent bisulphide of carbon; the sounds still continued. For the transparent bisulphide I then substituted the same liquid saturated with dissolved iodine. This solution cut off the light, while allowing the rays of heat free transmission; the sounds were immediately stilled.

Iodine vaporized by heat, in a small flask, yielded a forcible sound, which was not sensibly affected by the interposition of transparent bisulphide of carbon, but which was completely quelled by the iodine solution. It might, indeed, have been foreseen that the rays transmitted by the iodine as a liquid, would also be transmitted by its vapor, and thus fail to be converted into sound.*

To complete the argument. While the flask containing the bromine vapor was sounding in the intermittent beam, a strong solution of alum was interposed between it and the rotating disc. There was no sensible abatement of the sounds with either bromine or iodine vapor.

In these experiments the rays from the lime-light were converged to a point a little beyond the rotating disc. In the next experiment they were rendered parallel by the mirror, and afterwards rendered convergent by a lens of ice. At the focus of the ice lense the sounds were extracted from both bromine and iodine vapor. Sounds were also produced after the beam had been sent through the alum solution and the ice-lense conjointly.

With a very rude arrangement I have been able to hear the sounds of the more active vapors at a distance of 100 feet from the source of rays.

Several vapors other than those mentioned in this abstract have been examined, and sounds obtained from all of them. The vapors of all compound liquids will, I doubt not, be found sonorous in the intermittent beam. And, as I question whether there is an absolutely diathermanous substance in nature, I think it probable that even the

*I intentionally use this phraseology.

vapors of elementary bodies, including the elementary gases, when more strictly examined, will be found capable of producing sounds.

Note.—With some of the strongest sounds, which were audible when the ear-tube was entirely withdrawn from the ear, I tried to obtain the agitation of soap film. A glass tube, blown into a shape somewhat resembling a tobacco pipe, had its mouth closed by such a film, while its open stem was connected with a sounding flask. I did not succeed in producing any visible agitation. When the film was uniformly illuminated, or when it had become thin enough to produce iridescent colors, on holding a high-pitched tuning-fork near the open end of the stem the whole surface of the film was immediately covered with concentric rings, having the centre of the film for their centre. This belongs to the class of effects so vividly described by Mr. Sedley Taylor. A fork of the pitch of the sounding gas produced no visible effect upon the film.

THE MOON OF EARTH AND JUPITER.

By Pliny Earle Chase, LL.D.
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One of the most interesting chapters in Laplace's "Mecanique Celeste" is the one which describes tendencies to synchronism, among Jupiter's satellites, similar to those which are well known to watchmakers, in timekeepers which are suspended near each other. Some recent investigations in photodynamics have led me to the discovery of the following additional harmonies, which serve to connect Callisto, Jupiter's outer satellite, with the Earth as well as with Jupiter. They are due to the fact that Callisto represents the centre of gravity of Earth and Jupiter, when they are in conjunction.

1. Earth's accelerated rotation is to acceleration by condensation to a centre of linear oscillation as Jupiter's radius of accelerated rotation is to Callisto's radius of accelerated rotation. This relation is shown by the following proportion:

$$366{\cdot}2565 : 9 :: 4332{\cdot}5848^{\frac{3}{4}} : 16{\cdot}6891^{\frac{3}{4}}.$$

2. Jupiter's secular mean perihelion distance from Earth is to Callisto's mean distance from Jupiter as Jupiter's mass is to Earth's mass. This is shown in the following proportion:

$$3{\cdot}978245 : {\cdot}012585 : : \tfrac{1}{1047} . \tfrac{1}{88} : \tfrac{1}{331245}.$$

This proportion furnishes data for the following approximation to the distance of the Sun.

An isochronous radius is the mean distance at which a satellite would revolve about the planet in the same time as the planet revolves about the Sun. The cube root of the quotient of Sun's mass by planet's mass, multiplied by the isochronous radius, is $3962{\cdot}8 \times \left(\frac{31558149 \text{ sec.}}{5073{\cdot}6 \text{ sec.}}\right)^{\frac{2}{3}} = 1340291{\cdot}5$ miles; $331245^{\frac{1}{3}} \times 1340291{\cdot}5 =$ 92736000 miles.

3. Jupiter's secular perihelion radius vector is to Earth's semi-axis major as Callisto's semi-axis major is to Moon's semi-axis major.

$$4{\cdot}886325 : 1 : : {\cdot}012585 \times 92736000 : 238847.$$

Lockyer gives, for Moon's distance,				.	238793
Chambers	"	"	"	.	238830
Searle *	"	"	"	.	238870
Newcomb	"	"	"	.	240300

4. The radius of incipient nebular condensation is to the radius of secondary condensation (Callisto's semi-axis major) as the velocity of light is to the velocity of infinite fall.

$$6{\cdot}130992 : {\cdot}012585 : : 186282 \text{ miles} : 382{\cdot}378 \text{ miles}.$$

The "radius of incipient nebular condensation" represents the photodynamic energy which determined the positions of Neptune, Jupiter and Earth, with respect to Sun, and which also determined gravitating velocity.

Haverford College, Feb. 8, 1881.

Steam Carriages.—A steam carriage has been used for some time in Berlin. The Leipzig *Gazette* mentions that another German city, Chemnitz, the manufacturing centre of Saxony, with a population of about 50,000, is also using a steam car for the transport of merchandise through the streets without the use of rails. In two months it made forty-four trips, carrying 184,395 kilos (406,521 lbs.), which were easily distributed in all parts of the city, on grades and curves as well as on levels, without causing any accident to vehicles or pedestrians.—*Chron. Industr.* C.

* On the authority of von Littrow.

The Saharan Sea.—Commandant Roudaire has finished the investigations, which were indicated by the commission of the French Academy, in relation to the filling of the Tunisian and Algerian chotts by the sea. His conclusions are entirely favorable to the project, and would lead to the establishment of an interior sea, 400 kilometres (248·55 miles) long and 1600 kilometres (994·2 miles) in circumference.—*Comptes Rendus.* C.

New Lubricants.—K. Drechsler mixes graphite thoroughly with the whites or yolks of eggs, dries the mixture, pulverizes it and scatters it upon the parts of machinery which move slowly. G. Lieckfeld mixes graphite with soluble glass, so as to make a stiff broth. The mass is spread upon worn surfaces, where it soon hardens and can be filed or turned, so as to restore the machinery to its original perfection. —*Dingler's Journal.* C.

Difference in Galvanic Batteries.—Fr. Exner gives the following reasons why a Daniell battery does not show the influence of free oxygen so evidently as a Smee: 1. a concentrated solution of blue vitriol absorbs considerably less oxygen than acidulated water; 2. the hydrogen, which rises from the decomposition of the water, is abundantly supplied with $CuSO_4$ for the reduction of the copper; 3. it is well known that a Daniell element, if it remains long open, possesses a somewhat greater electro-motive force than if it was kept closed. This is an indication that the difference between a Daniell and a Smee element is only quantitative and not qualitative.—*Wiedem. Ann.* C.

The Place of Boron in the Series of Elements.—A. Etard gives satisfactory reasons for substituting glucinium, in the tabular position which Mendelejeff has assigned to boron. He finds that by the action of zinc ethyl upon boric ether we get triethylbromine, which, by its properties and its composition, corresponds very completely with triethylphosphine. After comparing successively the properties of boron and the phosphorus groups of elements, he unhesitatingly places boron at the head of the vanadium group, very near the phosphorus group. The resemblance of these two groups has already been established by Deville and Troost for niobium and tantalum, and by Roscoe for vanadium. These bodies, like boron, give volatile chlorides and oxychlorides, the density of which has already been measured, and they exhibit various other evidences of similarity in their respective salts and acids.—*Comptes Rendus.* C.

Origin of Phenol in Animals.—Prof. R. Engel, in studying the transformations of phenol in the animal economy, finds that the animal economy itself is the product of the pancreatic digestion of albuminoid substances, and of the putrefaction of those substances in the intestines.—*Ann. de Chim. et de Phys.* C.

Spectroscopic Study of the Sun.—L. Thollon is carefully studying the sun's surface at the Paris observatory. He describes a number of interesting hydrogen explosions, resembling gigantic displays of fireworks, and sometimes remaining visible for two days. One of them was more than 200,000 miles in height, and explosions of from 25,000 to 50,000 miles in height are quite common..—*Comptes Rendus.* C.

Electric Discharges in Gases.—E. Wiedemann has investigated the thermal and optical phenomena of gases under the influence of electric discharges. He concludes that specific changes of quality and quantity in the electric spectrum of a gas must be represented by equal quantities of energy in each molecule, which are independent of the pressure of the gas and of the diameter of the tube.—*Wiedemann's Annalen.* C.

Accurate Longitudes.—Admiral Mouchez commends the accuracy of Messrs. Green and Davis, in their determinations, by the transatlantic telegraph, of some of the principal longitudes on the coast of Brazil. He was himself charged, in 1860, with the task of determining the hydrography of Brazil and La Plata. In the performance of his duties he made a large number of astronomical observations, which accorded so closely with previous records that he affirmed in his report that the longitudes were known with nearly as great accuracy as those of the great observatories of Europe. The French astronomers, however, discredited his results and adopted values about 30 seconds greater. The American observers have fully confirmed Mouchez's assertions, showing that his greatest error was only 2·34 seconds, and in one case the error was only five-eighths of a second. Errors of from 2 to 4 seconds have been recently detected in the longitudes of some of the European ports. These comparisons also show the great precision with which longitudes can be ascertained by chronometers, when their rates are properly verified at the end of every voyage.=*Comptes Rendus.* C.

Filtration by Spongy Iron.—Messrs. Easton and Anderson are testing the value of spongy iron upon a large scale in the filters of the water works at Antwerp. The water is first allowed to pass through a mixture of iron and gravel covered with sand, when it passes into a second basin, the bottom of which is covered with sand. The experimental results have been more satisfactory than those from ordinary filters, and there are no indications of any necessity for renewing the iron, which serves to oxidize the organic matters suspended in the water.—*Ann. des Ponts et Chauss.* C.

Fusion of Metals by Electricity.—M. Imbert describes Siemens' method of fusing large metallic masses by means of electricity. He uses a plumbago crucible, surrounded by a thick refractory wall, the cover being traversed by a carbon rod of 20 millimetres (·79 inch) diameter. This rod is suspended by one of the arms of a balance beam, the other arm carrying a cylinder of soft iron sliding freely in a solenoid and plunging into a liquid, in order to moderate the oscillations which might arise from sudden variations of current. In one experiment 500 grammes (1·102 lbs.) were melted into a compact ingot in 4½ minutes. In melting large quantities the electrical method is rather more than twice as costly as the ordinary furnace, but for the fusion of precious or refractory metals, for chemical purposes, and for other applications where the question of economy is secondary, the new method is very convenient and practical. In melting small quantities it may even prove economical.—*Ann. du Gen. Civ.* C.

Book Notices.

MANUAL OF CATTLE FEEDING: A Treatise on the Laws of Animal Nutrition and the Chemistry of Feeding Stuffs in their Application to the Feeding of Farm Animals. With illustrations and an appendix of useful tables. By Henry P. Armsley, Ph.D., Chemist to the Connecticut Agricultural Experiment Station. 12mo. John Wiley & Son, New York.

This work belongs to the higher class of publications demanded by the rapid advance of agriculture. It treats not only of the best practice of feeding, but also of the theory, and gives us the results obtained at European experiment stations. The chemistry and the

physiology of the nutrition of domestic animals are clearly explained in order to enable the attentive reader "to adapt his practice to the varying conditions in which he may be placed; and more important still, to follow or take part in the advance of the science."

Part I of the work treats of the general laws of animal nutrition, in which not only are the values of the nitrogenous, non-nitrogenous and inorganic nutrients discussed, but the structure and functions of the organs of digestive circulation and respiration are briefly and very clearly described. The method of determining the nutritive effect of a ration and the part it plays in the formation of flesh and fat and in the production of work, are also well treated.

Part II is devoted to the feeding stuffs and their digestibility. In treating of the coarse fodders, and the method of curing them, ensilage is not forgotten, and the author's opinion of the process may perhaps be learned from the following quotations: "Ensilage of itself adds nothing to the value of the fodder submitted to it, but rather diminishes it. In the *sils* a sort of fermentation is carried on at the expense of the extractive matter of the fodder, resulting in the fermentation of various organic acids and volatile bodies and naturally diminishing the quantity of (non-nitrogenous) extract, and thereby increasing the percentage of all the other ingredients. Corn being a comparatively cheap crop, the loss of material during the fermentation (of the fodder) might be compensated by the improved quality of the residue." In the chapter on the concentrated fodders the following analyses of American linseed and cotton-seed cake are given, the samples having first been deprived of water.

	Ash.	Protein.	Crude fibre.	Non-nitrogenous extract.	Fat.
Linseed cake, per cent.,	7·13	32·48	9·70	37·66	13·03
Cotton-seed cake, "	7·33	46·17	7·98	19·98	18·54

But one sample of linseed oil-cake, from which the oil had been been extracted under the Simonin patent employed in this city, appears to be mentioned; that contained 3·15 per cent., while the protein was 39·92 per cent) Our author adds: "Cotton-seed cake, it will be seen above, is considerably richer in protein and fat, and poorer in (non-nitrogenous) extract than linseed cake, and must have correspondingly higher feeding value."

The feeding of farm animals is especially treated in Part III, of which the chapter on "feeding standards" is the first. By this term

is meant the quantities and proportions of the nutrients required for the several races of animals when the object is maintenance, fattening or work. Nutrients of a ration are divided into nitrogenous and non-nitrogenous, and the proportion of the former to the latter is called "the nutritive ratio." The ordinary day's work of a horse being taken as equal to the raising of 1,500,000 kilogrammes one metre high, the feeding standard is given in lbs. per 1000 lbs. live weight.

Feeding Standard Horse.

Digestible nitrogenous principles (protein), . .	1·8 lbs.
Digestible non-nitrogenous principles (carbhydrates and fat),	11·8 "
Nutritive ratio, ...	1 : 7
Total matter, freed from moisture. . .	22·5 lbs.

Tables are given showing the average composition and digestibility of the common feeding stuffs, and also the range of variation in this respect as far as observed. From these tables may be ascertained how much digestible matter is available in the feed we are using, and how much must be added to or taken from this ration in order to bring it to the feeding standard. On the subject of which it treats the book is far in advance of any other American publication we have read.

A. L. K.

BALDWIN LOCOMOTIVE WORKS. Illustrated Catalogue of Locomotives. Second edition. Royal 8vo. Philadelphia: 1881.

Of this catalogue it is impossible to speak in terms of too much praise. It gives a more varied and exact summary of the history of the locomotive than has heretofore appeared in any catalogue in this country, and more possibly than has appeared in all the other catalogues yet issued upon this subject. The whole is contained in a large 8vo volume of 152 pages, with eighteen photographs and eleven wood-cuts of their different types of locomotives; also sixteen wood-cuts descriptive of the historical part.

On page 96 will be found a cut of their "Single Fast Passenger Locomotive," similar to locomotive "5000," the 5000th of their build, which was originally intended for the Philadelphia & Reading Railroad Company, to run on the Bound Brook route, between Philadelphia and New York. It has made the distance, 89·4 miles, in 98 minutes, being at the rate of 54 miles an hour.

On page 68 also will be found a photograph of what is known as the "American" express passenger locomotive, there being more of

this type in use than any other style. One of the pattern known as 8—28C, having cylinders 17 x 22 inches and drivers 66 inches diameter, has pulled a train of 456,725 pounds (including locomotive and tender of 132,000 pounds), fifty-nine miles, counting one stop and one slow-down on drawbridge, in seventy-five minutes. This class is also largely in use for light freight trains. On page 103 is a cut of their celebrated ten-wheel freight locomotive known as 10E "Consolidation," which are now in extensive use for heavy freight trains on all the leading roads in America.

The first locomotive built in this country had but *one pair of driving wheels*, as in the "Old Ironsides," page 7 of catalogue. The experiments made with her, Mr. Baldwin says, "were eminently successful, realizing the sensation of a flight through the air of fifty or sixty miles an hour." The Old Ironsides, in its general arrangements, was a close copy of the English locomotives brought to this country in 1831, and up to 1840 was the leading type of "Fast Passenger Engine."

In 1842 the increase of business on railroads demanded more powerful locomotives, and it was found that for heavy freight business one pair of drivers was insufficient, and recourse had to be made to *coupling* an additional pair of wheels to secure the necessary adhesion so as to be able to pull the increased weight.

This catalogue is well written, and its description of the leading improvements in the construction of the locomotive, from its conception in America to the present day, will be found both interesting and instructive. In mechanical execution the volume leaves nothing to be desired, and it will be always preserved for the valuable information it contains. W. B. LeV.

Franklin Institute.

HALL OF THE INSTITUTE, March 16th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 102 members and 87 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and

announced, that at their last meeting 13 persons were elected members of the Institute; also that, by the resignation of Prof. Elihu Thomson, a vacancy existed in the Board. The President stated that filling this vacancy was the privileged business of the evening, and he therefore invited nominations. Prof. Wm. D. Marks and Mr. W. H. Thorne were placed in nomination, and tellers being appointed, they announced that Prof. Marks had received a majority of the votes cast, and he was thereupon declared a member of the Board for the unexpired term of Prof. Thomson.

The Secretary reported the following donations to the Library:

Airy—Mathematical Tracts. Cambridge, 1826.
Miller—Differential Calculus. Cambridge, 1837.
Bertrand—Analyse Mathematique. Paris, 1867.
Delaunay—Progresse de l'Astronomie. Paris, 1867.
Booth—Elliptic Integrals. London, 1851.
Nautical Almanac for 1870. London, 1866.
Frost and Wolstenholme—Solid Geometry. Cambridge, 1838.
Catalan—Theorèmes, etc., de Geometrie. Paris, 1865.
Salmon—Curves. Dublin, 1852.
Salmon—Algebra. Dublin, 1866.
Drew—Conic Sections. 1862.
Euclid's Geometry. Oxford, 1862.
Mulcahy's Geometry. Dublin, 1862.
Salmon's Geometry. Dublin, 1865.
Salmon's Conic Sections. London, 1863.
Godfrey's Astronomy. Cambridge, 1866.
Wilson's Geometry. 2 vols. Cambridge, 1868.
Bradley's Geometry. London.
Main's Astronomy. Cambridge, 1868.
Hann's Calculus. London, 1850.
Cheyne's Planetary Theory. Cambridge, 1862.
Godfrey's Lunar Theory. Cambridge, 1859.
Boole's Differential Equations. Cambridge, 1860.
Boole's Calculus of Finite Differences. Cambridge, 1860.
Cox's Integral Calculus. London, 1852.
Woolhouse—Differential Calculus. London.
Airy's Popular Astronomy. London, 1868.
Lockyer's Lesson's in Astronomy. London, 1868.
Ferrer's Trilinear Co-ordinates. London, 1866.
Taylor's Geometrical Conics. Cambridge, 1863.
Wolstenholme's Mathematical Problems. London, 1867.
Airy's Error of Observations. Cambridge, 1861.
Drew's Conic Sections. London, 1864.

Lagrange's Mecanique Analytique. Paris, 1853–5.
Parkinson's Treatise on Mechanics. Cambridge, 1863.
Tait and Steele's Dynamics. Cambridge, 1865.
Wurtz's Chemical Theory. London, 1869.
Airy—Atmospheric Vibrations. London, 1868.
Parkinson's Optics. London, 1866.
Regnault and Strecker's Chemistry. Braunschweig, 1869.
Newth's Natural Philosophy. London, 1868.
Quet—Electricity, Magnetism, etc. Paris, 1867.
Airy's Optics. London, 1866.
Bertin's Thermodynamics. Paris, 1867.
Routh—Dynamics of Rigid Bodies. London, 1868.
Desain's Theorie de la Chaleur. Paris, 1868.
Airy's Gravitation. London, 1834.
Whewell's Dynamics. Cambridge, 1832.
Frost's Newton's Principia. Cambridge, 1863.
Whewell's Newton's Principia in Latin. London, 1846.
Evans' Newton's Principia. Cambridge, 1855.
Weinhold's Vorschule der Experimentalphysik. Leipzig, 1871.
Lloyd's Wave Theory of Light. London, 1857.
Hirn's Thermodynamics. Paris, 1868.
Besant's Hydromechanics. Cambridge, 1867.
Helmholtz's Optics. Leipzig, 1867.
Lunn's Motion. Cambridge, 1859.
Thorpe's Chemical Problems. London, 1870.
Besant's Hydrostatics. Cambridge, 1867.

Presented by Mr. Alexander E. Outerbridge, United States Mint, Philadelphia.

History of Coaches. By G. A. Thrupp. London, 1877.
From the Author.

Mr. T. Mellon Rogers read the paper announced for the evening, on "The Concentration of Ores," and also exhibited his new Swing Gate Concentrator. It is a novel apparatus for washing and concentrating gold, silver and other metals in ores of low grades. The machine consists of a water tank, divided into two compartments, in one of which is suspended a box with screen for holding the ore, while the other takes the waste. In the bottom of each are hoppers with eduction pipes. In the main compartment is a vibrating gate, provided with valves, and moved by a rod and adjustable eccentric, driven by steam power. The forward swing of the gate moves the large body of water in advance of it upward through the sieve, while at the same time a partial vacuum is produced in the smaller body of water

behind the gate, thus operating the valves which are opened and closed by variations in the pressure of the body of water on either side of them. Mr. Rogers exhibited the machine at work, and by the aid of drawings on the screen explained its operation. He said that there were many low grade ores, particularly in North and South Carolina and Virginia, that could not be profitably worked without concentration. If we could get a liquid of such density as to allow the ore to sink and the gangue to float, concentration would be very simple; but, as we have not, we must employ the best available substitute, which is water. Very rude apparatus is generally employed for concentrating the ores, and is really efficient, since the operator can vary the degree of agitation to be given it according to the ore. The apparatus devised by Mr. Rogers is designed to effect the same purpose by the use of an eccentric, readily adjustable to give changes of speed, by which to treat fine or coarse ores. The machine was exhibited in operation concentrating an ore containing argentiferous galena and gold sulphuret. Mr. Rogers also exhibited samples of concentrations made by him. In answer to questions he said that some of these machines were in use; that with meshes of from forty to sixty, eight tons could be concentrated in a day, and with larger meshes from eight to twelve tons, and that one important feature was the use of screws for leveling the box in which the ore is placed.

Mr. Tatham asked what advantage there was in the use of a swinging gate over an ordinary plunger.

Mr. Rogers replied that the swinging gate put the greatest agitation of the water where it would do the most good, and prevented a washing over and waste of ore that would result from a uniform raising of the water through the meshes.

The Secretary called attention to the system of underground telegraphy, invented by Restore B. Lamb and owned by the National Subterranean Electric Company. Terra cotta blocks, through which there are a number of small holes, glazed inside and out, are used. The pipes or blocks are joined together, much like sewer pipes; and cemented, the holes being continuous. Cables of wires, enclosed in rubber tubes, are carried through the holes. At convenient distances along the route are masonry chambers underground, entered from a manhole, in which the sections of tubular blocks terminate. In these the connections between two sections and repairs are made, the wires

or cables requiring repair being drawn through into the chamber, and then replaced. The wires are amply protected from the weather, and thoroughly isolated from each other, and, having once been laid, the streets through which they pass do not have to be disturbed to add additional wires as required, or to make repairs. Satisfactory experiments with the system have been made in Camden, where the "blocks" have been laid through swampy "made" ground for several months.

Several inquiries were made about the manner of laying the wires, the practical operation of the system, etc.

Mr. Patrick said there was some misapprehension apparently about the way in which the wires were drawn through the tubes. As he understood it the blocks were laid first with single leading wires through the several holes with which to draw through the cables from section to section and replace them. It is therefore not necessary to put in the whole number of wires which the blocks could carry at once; the number can be increased at will without disturbing the streets. The wires laid can be used for telephones, telegraphic or electric lighting.

Fleischmann's improved medical battery, which was shown, has a peculiar interrupter of the current by which alternate contraction and relaxation of the muscles may be obtained when the battery is used for medical purposes. The interrupter is a horizontal swinging lever—a flat spring about five inches long, very flexible at the secured end and having a weighted armature on the other end to be attracted by the electro-magnet. As it swings to and fro horizontal contact is made and broken by the spring. The pulsations are regular and can be varied from 60 to 300 per minute. It requires very small attractive power in an electro-magnet to keep it in motion. The battery also has an interrupter which may be used by itself. A modified Grenet battery is used.

T. Angell's portable vaccum steam heater, made by James P. Wood & Co., was shown by model. In the hot air chamber are a number of tubes closed at both ends and exhausted of air, but containing a small quantity of water. Each tube is independent of the others, and each is hermetically closed. A small part of the tube extends into the fire-chamber, the greater part being in the hot air chamber, which has the usual induction and eduction air flues.

The American star bicycle was exhibited by the manufacturers, Messrs. Pressly and Crowell. It was invented by G. W. Pressly, and